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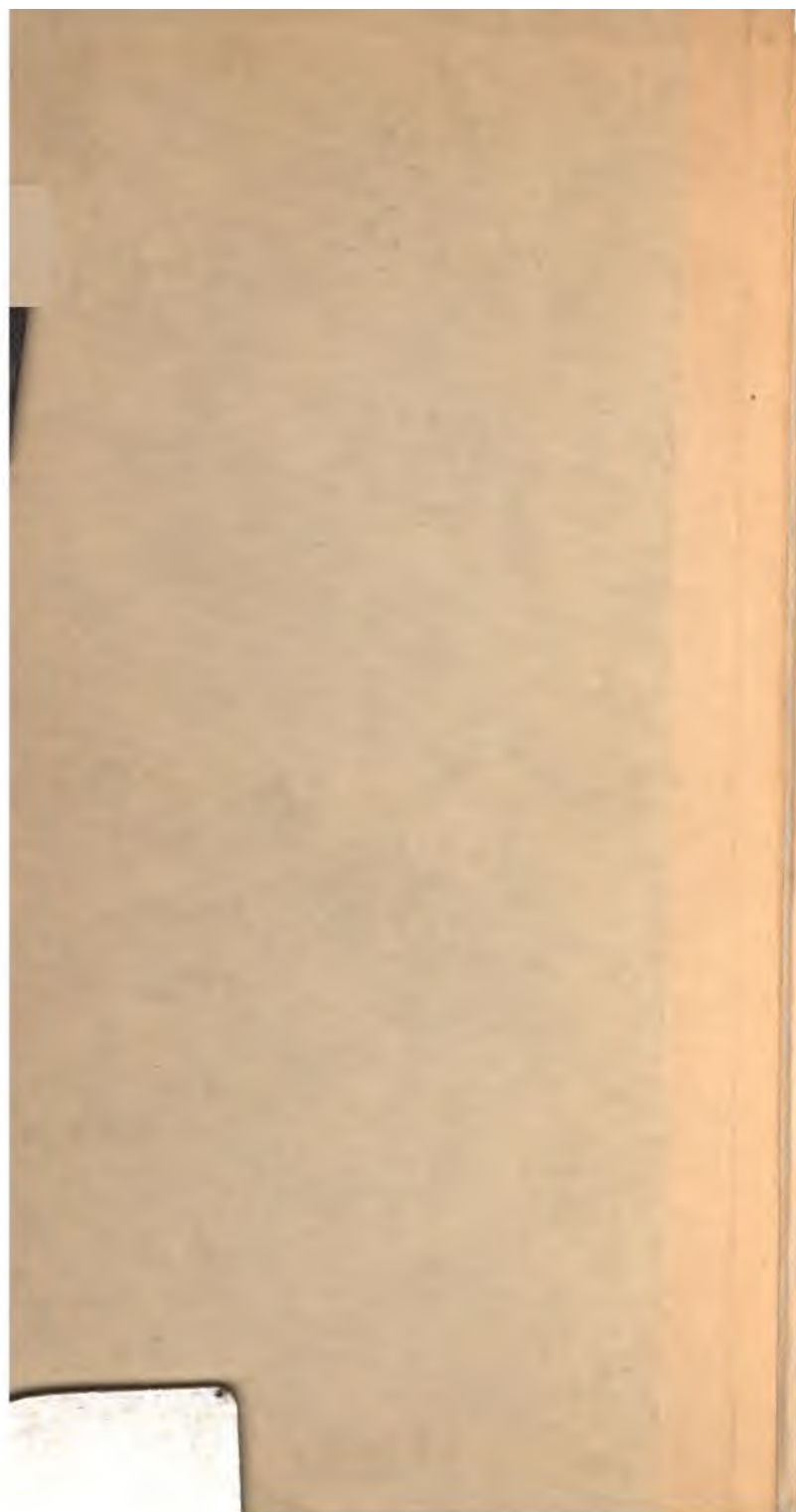
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VOL. VII.

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BY WILLIAM NICHOLSON.

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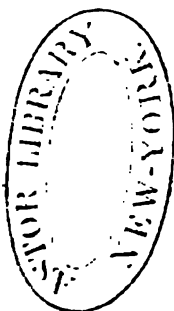
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THE Authors of Original Papers in the present Volume, are Joseph Huddart, Esq. F. R. S.; John Dalton; Everard Home, Esq. F. R. S.; Mr. John Gough; Robert Briggs, M. D.; Mr. Ezekiel Walker; Mr. John Farcy; Mr. Frederick Accum; Sir A. N. Edelcrantz; Richard Chenevix, Esq. F. R. S. and M. R. I. A.; Mr. R. Winter; Mr. James Stodart; Mr. T. Jones; W. H. Woollaston, M. D. F. R. S.; J. M. Elliott; Mr. William Close; Mr. W. Jones, F. Am. P. S.; C. Wilkinson, Esq.; Mr. J. C. Hornblower; Mr. J. J. Hawkins; I. R. I.; Mr. Sharples; B.

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Of Engravings the Subjects are, 1. Station Pointer for determining the precise Situation of a Ship or Vessel from two Horizontal Angles, by Joseph Huddart, Esq. F. R. S. 2. Mr. Dalton's Scheme for shewing the Distribution of the Particles in Mixed Gases. 3. Diagram to shew the Method of constructing and computing the Situation of an Observer from

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from the Angular Position of Three known Objects. 4. Double Barrel and Winch for encreasing the Power without Diminution of Strength. 5. Screw Press, which by the contrary Action of Two Helical Threads, affords great Increase of Power at the Place of most extreme Pressure. 6. Pump acting by the Difference of Two Cylinders. 7. Alphabetical System of Writing in the Dark, by Mr. John Gough. 8, 9. Two Plates, Apparatus for supplying Worm-Tubs, &c. with Water, on the Syphon Principle; By Sir A. N. Edelcrantz. 10, 11. Construction of a simple Repeater for the Hours and Quarters, By Mr. J. M. Elliot. 12. Diagram to illustrate the Investigations of the Figure of the Earth, by Professor Playfair. 13. Improved Chemical Furnace, by R. Chenevix, Esq. F. R. S. 14. Figures to illustrate an Improvement in Spectacles, by W. H. Wollaston, M. D. F. R. S. 15. New Steam Digester, in which the Heat and Pressure are rendered stationary; by Sir A. N. Edelcrantz. 16. Remarkable Strata of Flint, by Sir Henry Charles Englefield, Bart. F. R. S. 17. Figure to illustrate Dr. Herschel's Theory of the Changes in the Situation of Double Stars. 18. Mr. Hornblower's Apparatus for sweeping Chimnies. 19. Figures to explain the Changes of Double Stars, by Dr. Herschel. 20. Galvanic Apparatus to afford the Maximum of Effect in Combustion, by I. R. I. 21. Hydraulic Engine for raising Fresh Water, by the Discharge of Waste Water, by Mr. Sharples.

Soho Square, April, 1804.

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JANUARY, 1804.

ARTICLE I.

Description and Use of the Station Pointer; an Instrument for readily ascertaining the Situation of the Observer after having determined the angular Position of three known Objects. Constructed by JOSEPH HUDDART, Esq. F. R. S. and communicated by him to the Editor.

I HAVE long been desirous of presenting my readers with a drawing and account of the instrument which forms the subject of the present paper. Its utility in protracting the situation of a vessel upon a chart from an accurate observation by the sextant of the angular position of three known objects on shore, instead of the uncertain and rough method of bearing by compass, is great and obvious; and if any other recommendation could be desired, it would be that of its repeated application in the hands of a man of science and experience. This we have the satisfaction to possess in the public labours of Mr. Huddart, whose maritime surveys are too well known and esteemed to require the suffrage of any individual at this day. The very first occasion, when I happened to have the pleasure of conversing with him, I requested the communication of this instrument, with which he with great readiness favoured me, and from which the drawing exhibited in *Plate I.* was made.

Description.
It is a protractor with three radii, which being set and duly placed on a chart gives the station of the observer.

It consists of a circle divided into 360° and filled with three radii, C B, C D and C A; the first of which is fixed at zero and the other two are moveable, and have each a nonius shewing single minutes of a degree. The centre at C is perforated in order to admit the pointer L, which is cylindrical and well fitted to the hole, where it serves to give a correspondent mark upon the chart or paper when in use. I is a magnifier carried by an arm, having its reflector K to throw light on the divisions, and its cylindrical stem H to be inserted in the hollow centre at C. As it must, while in this situation, prevent the introduction of the steel point L, the stem H is itself perforated in order to allow a smaller steel point M to be introduced for the same purpose. The radii A B and D are of brass; but they may be prolonged to an extent that would render them heavy and inconvenient if of metal, by the addition of three rulers of wood, which are attached when required by milled head screws and steady pins, as shewn by breaking the radius A, and also in the side view at G.

The arms D and B are capable of being brought very near each other, and consequently of being set to a very small angle; but A cannot, on account of the noniuses, be brought within ten degrees of D. Whenever, therefore, the left hand angle happens to be small, it becomes necessary to carry the arm A round, until by its situation on the other side of B, it comes to be employed to take the right hand angle, and consequently leaves the left to be taken between D and B.

The pointer A is provided with a clamp, by means of which, that arm can be first secured in its place, and the pointer D is then set without fear of disturbing the situation of the arm first set. It seldom happens that both angles are small in maritime surveying; so that the condition that A cannot be brought very near either of the other arms, has not been found productive of inconvenience.

In *Fig. 1. Plate III.* let A, G, and M represent three objects, of which the actual distances from each other are known and projected in a chart. Suppose an observer at H to have measured the angle $A H M = 22^\circ.40'$, and the angle $M H G = 53^\circ.59'$ it is required to protract the place H by means of the station pointer.

Solution. Set the arms of the instrument to the respective angles, and apply them to the chart so that each point A, M, and

and G, shall be found upon the fiducial edge of its respective pointer. The center will then be found at H, which may be marked by the steel point.

Description and
use of the station
pointer.

Rationale of the Process. When an observer at H sees M and A at the angular distance of $22^{\circ}.40'$ from each other, he must himself (by Euclid III. 20. 21.) be placed somewhere in a circle MAH, in which the line AM is the chord of twice that angle. And so likewise when he observes M and G under the angle $53^{\circ}.59'$ he must be placed in another circle MGH, of which MG is the chord of twice that angle. Consequently H must be at a point common to both circles; namely where they intersect. The more direct the intersection the less will the result be affected by the errors of observation:—when the intersections are very oblique, the result will in practice become uncertain; and in the case where the two circles coincide throughout, the determination by two angles will be no better than by one; that is to say H may be any where in that circle. This last case occurs when a circle drawn through all the objects does also pass through the station itself.

In the figure already referred to H, denotes the west garret window of my house in Soho-square, and the letters A, G, and M respectively denote the steeples of the parish churches of St. Anne Soho*, St. Giles, and St. Martin, laid down by their difference of latitude and departure from St. Paul's church, obtained from the bearings and distances in General Roy's Survey's in the Philosophical Transactions, or the *Account of Operations carried on for accomplishing a Trigonometrical Survey of England and Wales, &c. published in quarto by Captain Mudge and Mr. James Dalby. London, 1799, with 22 plates.*

The table is at pages 194, 195 of the last mentioned work. Whence and by observation with Hadley's Sextant, the data are: From St. Paul's

St. Anne's bears S. $86^{\circ}.9'.59''$ W. 7754 feet.—Southing 518.4—Westing 7736.6 feet.

St. Martin's S. $74^{\circ}.28'.59''$ W. 6748 feet.—Southing 1805.2.—Westing 6502.1 feet.

St. Giles S. $94^{\circ}.36'.28''$ W. 6917 feet.—Northing 55.6—Westing 6894.7 feet.

* The steeple of St. Anne's church has been since rebuilt, and stands in the same latitude, but nine inches farther to the East than before.

Description and use of the station pointer. Angle observed between St. Giles's and St. Martin's from Soho Square $22^{\circ} 40' \frac{1}{2}$.

Angle observed between St. Martin's and St. Anne's from ditto $53^{\circ} 59'$.

Construction. Draw the parallel of St. Paul's, and set off the points A, G, and M by the respective differences of latitude and of meridians to represent the churches of St. Anne, St. Giles and St. Martin. Join A and G, the two objects most remote in their bearings, and from the extremities A and G on the side of the line farthest from H, draw the lines Ah, Gh, making angles with A G respectively, equal to the angles observed on contrary sides of the line pointing to the middle object M. Through A, h and G describe a circle; and through M and h draw a right line, which prolonged will cut the circle in H. Join A H, G H, and the angles h A G, M H G, and A G h, M H A will be respectively equal, because standing on the same arcs h G and h A; that is to say, the objects will be seen from H under the observed angles, and consequently H will be the place of the house.

Computation. The numbers in the figure were had by careful construction on a scale of one inch to equal 100 feet, using a beam compass, which divides the inch into 1000 parts. But as the computation may not be unacceptable to beginners in trigonometry, I will here give the process as a conclusion to the present paper.

1. The triangle M A G is known. We have therefore the side A G and two angles given in the triangle A h G, which is thus determined.

2. In the known triangle M A G by subtracting the angle A G h from the angle A G M we gain the angle h G M which, with h G, G M, determines the triangle h G M.

3. The angle H h G is (by Euclid I. 32) equal to the sum of the interior opposite angles h G M, G M h. Therefore we have the side h G (by paragraph 1) and the two angles H and h in the triangle H h G. Consequently that triangle is known, and the distance from H, the house, to the object G may be had.

4. In the triangle M G H the angle at G is equal to the sum of the known angles M G h, h G H and the other angle at H and also the side M G are known. Whence the triangle is determined, and the distance from H to the object M may be had.

5. In

5. In the triangle MAH , the angle at M is equal to the difference of the known angles GMA , GMH ; and the angle at H , together with the two sides AM , HM are also known. Whence the third side HA or distance from the house to the object A may be found.

6. Lastly the bearing of any of the three sides of the triangle MAG may be had from the known difference of latitude and meridional distance of its extremities; And this bearing may with ease be then applied to shew the bearings of the lines HA , HM , HG .

W. N.

II.

Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat.
By JOHN DALTON.

(Concluded from Page 273, Vol. VI.)

ESSAY III.

ON EVAPORATION.

WHEN a liquid is exposed to the air it is gradually diffused in it. The process by which this effect is produced we call evaporation. Evaporation defined.

Many philosophers concur in the theory of chemical solution: atmospheric air, it is said, has an affinity for water; it is a menstruum in which water is soluble to a certain degree. It is allowed notwithstanding by all, that each liquid is convertible into an elastic vapour in vacuo, which can subsist independently in any temperature; but as the utmost forces of these vapours are inferior to the pressure of the atmosphere in ordinary temperatures, they are supposed to be incapable of existing in it in the same way as they do in a torricellian vacuum; hence the notion of affinity is induced. According to this theory of evaporation, atmospheric air (and every other species of air for aught that appears) dissolves water, alcohol, ether, acids, and even metals. Water below 212° is chemically combined with the gases; above 212° it assumes a new form, and Theory which ascribes evaporation to chemical solution in air—

—is obscure and difficult.

Theory of distinct elastic vapours in the atmosphere.

and becomes a distinct elastic fluid, called *steam*; whether water first chemically combined with air, and then heated above 212° , is detached from the air or remains with it, the advocates of the theory have not determined.—This theory has always been considered as complex and attended with difficulties; so much that M. Piclet and others have rejected it, and adopted that which admits of distinct elastic vapours in the atmosphere at all temperatures, uncombined with either of the principal constituent gases; as being much more simple and easy of explanation than the other; though they do not remove the grand objection to it, arising from atmospheric pressure. It has however been made to appear in these essays, I presume, that the objection to it from pressure, is itself founded upon an ungrounded hypothesis.

Leaving the theory of evaporation for the present, we shall proceed to the experiments.

The following positions have been established by others, and need therefore only to be mentioned here.

Positions. Different fluids evaporate with different rapidity. Evaporation is as the surface; it increases with the temperature; and when the air is in motion, and is drier.

1. Some fluids evaporate much more quickly than others.
2. The quantity evaporated is in direct proportion to the surface exposed, all other circumstances alike.
3. An increase of temperature in the liquid is attended with an increase of evaporation, not directly proportionable.
4. Evaporation is greater where there is a stream of air than where the air is stagnant.
5. Evaporation from water is greater the less the humidity previously existing in the atmosphere, all other circumstances the same.

The objects in view in this essay, are,

Objects of the present essay.

1. To determine the precise effect that a variation of temperature has upon the quantity evaporated.
2. To determine the ratio of evaporability of different fluids.
3. To find a rule by which the quantity and effect of previous humidity in the air may be ascertained.
4. From these and other facts to obtain a true theory of evaporation.

On the Evaporation of Water at 212°

Water boiled in a vessel for a definite time lost by evaporation a quantity,

I took a small cylindrical vessel of tin, its diameter $3\frac{1}{2}$ and depth $2\frac{1}{2}$ inches; and having fixed three pieces of wire to equidistant points of the circumference, they were fastened together

ther at the top, and the extremities bent into a hook, by which the vessel might be suspended from the end of a balance, &c. This done, the vessel was nearly filled with water, which was then made to boil over a small red fire in different circumstances: it was held in the hand and removed nearer to or further from the fire, so as to be kept just at the point of ebullition. In this state the vessel and water were weighed true to a grain, and the instant of time noted by a watch; then kept as above at 212° for ten minutes or more and again weighed: and the loss of water by evaporation, per minute, was thus ascertained. The experiments were repeated several times in the same as well as in different circumstances; and the results in no instance differed materially when obtained in the same circumstances.

The least evaporation per minute was 30 grains: this was when the fire, or lamp, was in the middle of a room, the doors and windows shut, and the air calm. —which was least when the surrounding air was stillest,

The next degree was 35 grains per minute or thereabouts: this was when the evaporating vessel was over a small fire in the usual fire-place; there being a moderate draught of air, and the room close. —and gradually more,

A brisker fire, causing a stronger current of air up the chimney, gave from 35 to 40 grains per minute. —the more quickly the air was changed.

When the windows of the room were open, and a strong wind prevailed, the draught over the fire was proportionally increased, and the evaporation was from 40 to 45 grains per minute.

The extremes that have thus been noticed are 30 and 45 grains per minute: but were the experiment tried in the open air in high winds, I am inclined to believe from a comparison of the observations, that an evaporation of 50, 55 or even 60 grains per minute might be observed.

On the Evaporation of Water below 212° .

I have frequently tried the evaporation at all the temperatures below 212° : it would be tedious to enter into detail of all the experiments, but shall give the results at some remarkable points. In all the high temperatures I used the vessel above mentioned, keeping a thermometer in it, by which I could secure a constant heat, or at least keep it oscillating within narrow limits. Water at temperatures below 212° lost by evaporation.—

The

The evaporation from water of 180° was from 18 to 22 grains per minute, according to circumstances; or about half of that at 212° .

Quantities
which were,

At 164° it was about one third of the quantity at the boiling temperature; or from 10 to 16 grains per minute.

At 152° it was only one fourth of that at boiling; or from 8 to 12 grains, according to circumstances.

The temperature of 144° afford $\frac{1}{5}$ of the effect at boiling; 138° gave $\frac{1}{6}$, &c.

—in every part
of the scale pro-
portional to the
force of vapour
at that temper-
ature.

Having previously to these experiments determined the force of aqueous vapour at all the temperatures under 212° , I was naturally led to examine whether the quantity of water evaporated in a given time bore any proportion to the force of vapour of the same temperature, and was agreeably surpris'd to find that they exactly corresponded in every part of the thermometric scale; thus the forces of vapour at 212° , 180° , 164° , 152° , 144° , and 138° are equal to 30, 15, 10, $7\frac{1}{2}$, 6 and 5 inches of mercury respectively, and the grains of water evaporated per minute in those temperatures were 30, 15, 10, $7\frac{1}{2}$, 6 and 5 also; or numbers proportional to these. Indeed it should be so from the established law of mechanics, that all effects are proportional to the causes producing them. The atmosphere, it should seem, obstructs the diffusion of vapour, which would otherwise be almost instantaneous, as in vacuo; but this obstruction is overcome in proportion to the force of the vapour. The obstruction however cannot arise from the weight of the atmosphere, as has till now been supposed; for then it would effectually prevent any vapour from arising under 212° : but it is caused by the *vis inertiae* of the particles of air; and is similar to that which a stream of water meets with in descending amongst pebbles.

In low temper-
atures the eva-
poration is as the
force of vapour
at the temper-
ature of the
water diminished
by that existing
in the atmos-
phere.

The theory of evaporation being thus manifested from experiments in high temperatures, I found that if it was to be verified by experiments in low temperatures, regard must be had to the force of vapour actually existing in the atmosphere at the time. For instance, if water of 59° were the subject, the force of vapour of that temperature is $\frac{1}{88}$ of the force at 212° , and one might expect the quantity of evaporation $\frac{1}{88}$ also; but if it should happen, as it sometimes does in summer, that an aqueous atmosphere to that amount does already exist, the evaporation, instead of being $\frac{1}{88}$ of that from boiling water, would

would be nothing at all. On the other hand, if the aqueous atmosphere were less than that, suppose half of it, corresponding to 39° of heat, then the effective evaporating force would be $\frac{1}{1.37}$ of that from boiling water; in short, the evaporating force must be universally equal to that of the temperature of the water, diminished by that already existing in the atmosphere. In order to find the force of the aqueous atmosphere I usually take a tall cylindrical glass jar, dry on the outside, and fill it with cold spring water fresh from the well; if dew be immediately formed on the outside, I pour the water out, let it stand a while to increase in heat, dry the outside of the glass well with a linen cloth, and then pour the water in again; this operation is to be continued till dew ceases to be formed, and then the temperature of the water must be observed; and opposite to it in the table (p. 264, Vol. VI.) will be found the force of vapour in the atmosphere. This must be done in the open air, or at a window; because the air within is generally more humid than that without. Spring water is generally about 50° , and will mostly answer the purpose the three hottest months in the year: in other seasons an artificial cold mixture is required.—The accuracy of the result obtained this way I think scarcely needs to be insisted upon. Glass, and all other hard smooth substances I have tried, when cooled to a degree below what the surrounding aqueous vapour can support, cause it to be condensed on their surfaces into water. The degree of cold is usually from 1 to 10° below the mean heat of the 24 hours; in summer I have often observed the point as high as 38° or 59° , corresponding to half an inch of mercury in force, and once or twice have seen it at 62° : in changeable and windy weather it is liable to considerable fluctuation; but this is not the place to enlarge upon it.

Experiment to determine the force of the atmospheric vapour.

It is the same as that of water, which is at the lowest temperature, which condenses no dew on its containing vessel.

For the purpose of observing the evaporation in atmospheric temperatures I got two light tin vessels, the one six inches in diameter, and half an inch deep, the other eight inches diameter and $\frac{1}{2}$ inch deep; and made to be suspended from a balance, like the former one. When any experiment designed as a test of the theory was made, a quantity of water was put into one of these (generally the six inch one, which I preferred) the whole was weighed to a grain; then it was placed in an open window or other exposed situation for ten or fifteen minutes, and again weighed to ascertain the loss by evaporation;

Narrative of the experiments of evaporation in atmospheric temperatures.

at

at the same time the temperature of the water was observed, the force of the aqueous atmosphere ascertained as above, and the strength of the current of air noticed. From a great variety of experiments made both in the winter and summer, and when the evaporating force was strong and weak, I have found the results, entirely conformable with the above theory. The same quantity is evaporated with the same evaporating force thus determined, whatever be the temperature of the air, as near as can be judged; but with the same evaporating force, a strong wind will double the effect produced in a still atmosphere. Thus, if the aqueous atmosphere be correspondent to 40° of temperature and the air be 60° , the evaporation is the same as if the aqueous atmosphere were at 60° of temperature and the air 72° ; and in a calm air the evaporation from a vessel of six inches in diameter in such circumstances would be about .9 of a grain per minute, and about 1.8 grains per minute in a very strong wind; the different intermediate quantities being regulated solely by the force of the wind.

Account of the
table of evapo-
ration.

The following table exhibits the ratios and quantity of water evaporated in each temperature, derived from the preceding theory, and confirmed by experiments, as far as they have been extended. The first column expresses the temperature; the second, the corresponding force of vapour taken from the preceding table; the other three columns give the number of grains of water that would be evaporated from a surface of six inches in diameter in the respective temperatures, on the supposition of there being previously no aqueous vapour in the atmosphere. These columns present the extremes and the mean of evaporation, likely to be noticed, or nearly such: for, the first is calculated upon the supposition of 35 grains loss per minute from the vessel of $3\frac{1}{4}$ inches in diameter; the second, 45 and the third 55 grains per minute.

TABLE

TABLE

Showing the force of vapour, and the full evaporating force of every degree of temperature from 20° to 85°, expressed in grains of water that would be raised per minute from a vessel of six inches in diameter, supposing there were no vapour already in the atmosphere.

Table of the quantities of water evaporated per minute at temperatures between 20° and 85°.

Temper- ature. 212°	Force of Vap. Inch. 30	Evaporating Force in Grains.		
		120	154	189
20	.129	.52	.67	.82
21	.134	.54	.69	.85
22	.139	.56	.71	.88
23	.144	.58	.73	.91
24	.150	.60	.77	.94
25	.156	.62	.79	.97
26	.162	.65	.82	1. 02
27	.168	.67	.86	1. 05
28	.174	.70	.90	1. 10
29	.180	.72	.93	1. 13
30	.186	.74	.95	1. 17
31	.193	.77	.99	1. 21
32	.200	.80	1. 03	1. 26
33	.207	.83	1. 07	1. 30
34	.214	.86	1. 11	1. 35
35	.221	.90	1. 14	1. 39
36	.229	.92	1. 18	1. 45
37	.237	.95	1. 22	1. 49
38	.245	.98	1. 26	1. 54
39	.254	1. 02	1. 31	1. 60
40	.263	1. 05	1. 35	1. 65
41	.273	1. 09	1. 40	1. 71
42	.283	1. 13	1. 45	1. 78
43	.294	1. 18	1. 51	1. 85
44	.305	1. 22	1. 57	1. 92
45	.316	1. 26	1. 62	1. 99
46	.327	1. 31	1. 68	2. 06
47	.339	1. 36	1. 75	2. 13
48	.351	1. 40	1. 80	2. 20
49	.363	1. 45	1. 86	2. 28
50	.375	1. 50	1. 92	2. 36
51	.388	1. 55	1. 99	2. 44
52	.401	1. 60	2. 06	2. 51
53	.415	1. 66	2. 13	2. 61
54	.429	1. 71	2. 20	2. 69
55	.443	1. 77	2. 28	2. 78
56	.458	1. 83	2. 35	2. 88
57	.474	1. 90	2. 43	2. 98

Table continued.

Table of the quantities of water evaporated per minute at temperatures between 20° and 85°.	Temper- ature.	Force of Vap. inch.	Evaporating Force in Grains.		
	212°	30	120	154	189
	58°	.490	1. 96	2. 52	3. 08
	59	.507	2. 03	2. 61	3. 19
	60	.524	2. 10	2. 70	3. 30
	61	.542	2. 17	2. 79	3. 41
	62	.560	2. 24	2. 88	3. 52
	63	.578	2. 31	2. 97	3. 63
	64	.597	2. 39	3. 07	3. 76
	65	.616	2. 46	3. 16	3. 87
	66	.635	2. 54	3. 27	3. 99
	67	.655	2. 62	3. 37	4. 12
	68	.676	2. 70	3. 47	4. 24
	69	.698	2. 79	3. 59	4. 38
	70	.721	2. 88	3. 70	4. 53
	71	.745	2. 98	3. 83	4. 68
	72	.770	3. 08	3. 96	4. 84
	73	.796	3. 18	4. 09	5. 00
	74	.823	3. 29	4. 23	5. 17
	75	.851	3. 40	4. 37	5. 34
	76	.880	3. 52	4. 52	5. 53
	77	.910	3. 65	4. 68	5. 72
	78	.940	3. 76	4. 83	5. 91
	79	.971	3. 88	4. 99	6. 10
	80	1. 00	4. 00	5. 14	6. 29
	81	1. 04	4. 16	5. 35	6. 54
	82	1. 07	4. 28	5. 50	6. 73
	83	1. 10	4. 40	5. 66	6. 91
	84	1. 14	4. 56	5. 86	7. 17
	85	1. 17	4. 68	6. 07	7. 46

Use of the table The use of this table will appear from the following problems:
familiarily il-
lustrated.

PROBLEM I.

Having given the temperature at which the aqueous atmosphere begins to be condensed into water, and the temperature of the air, to find the quantity of water that would be evaporated in a minute from a vessel of six inches diameter.

Solution. Subtract the grains opposite to the lower temperature from those opposite to the higher one, in the first, second or third column of grains, according to the strength of the wind, and the remainder will be the quantity evaporated in a minute under those circumstances, nearly.

Example

Example. Let the point of condensation be 52° , the temperature of the air 65° , with a moderate breeze.

Use of the table
familiarily il-
lustrated.

The number opposite 52° in the second column of grains is 2.06, and that opposite 65° is 3.16; the difference, 1.1 grain, is the evaporation per minute.

PROBLEM II.

Having given the quantity evaporated in a minute, found by experiment, and the temperature of the air, to find the force of the aqueous atmosphere, and the point of condensation.

Solution. Subtract the observed evaporation from that opposite the given temperature in the table; and look above for the number nearest to the remainder in the same column of evaporation, opposite to which will be found the force of the aqueous atmosphere, and the point at which it begins to be condensed.

Example. Finding the evaporation from a vessel of six inches in diameter to be 1.7 grain per minute with a brisk wind, air 62° ; what is the weight of the aqueous atmosphere, and the temperature at which it begins to be condensed into water?

The number opposite 62° in the third column of grains is 1.32, being the whole evaporating force at that temperature in a perfectly dry atmosphere; from which take 1.7 grains, the real evaporating force observed, and the remainder, 1.32, corresponds, as per table, to the force .294 inches of mercury, the weight of vapour, and to 43° of temperature.*

Evaporation of Spirits, Ether, &c.

If the law of evaporation above given apply to water in every part of the scale of heat, no reasonable doubt can be entertained respecting its application to other liquids. I have notwithstanding made several experiments on others, the results of which are conformable to the same law. Some of them follow:—

Experiments
which shew that
the evaporation
of spirits, ether,
&c. follow the
same law as
water.

* It may be proper to remind the reader that all the experiments on evaporation are understood to be made in the open air, or in a window with a current inward; also it may be observed the evaporation in a close room is much less and is besides irregular, being greater proportionably from a less surface, evidently from the stagnation of the air.

1. Spirit

Experiments which shew that the evaporation of spirits, ether, &c. follow the same law as water.

1. Spirit of wine.—Evaporated from a surface of four inches in diameter, 54 grains in 25 minutes: air 53° ; aqueous atmosphere at 49° , and beginning to rain with a moderate breeze. It would proportionally have been 121 grains from a vessel of six inches in diameter. This gives nearly five grains per minute. The same spirit boiled at or near 180° .

Now from the *data*, water of 83° is equivalent in force to spirits of 53° ; and it may be seen that the evaporating force of water of 83° is nearly 5 in the first and second columns of grains of the table. It seems probable that the aqueous atmosphere does not diminish the evaporation of spirits, as it does that of water.

2. Ether.—1. Put a phial containing ether, and a small tin vessel of $1\frac{1}{4}$ inch diameter into a scale and balanced them exactly: then poured the ether into the evaporating vessel and put the phial into the scale again; took out 40 grains from the opposite scale, and waited till the equilibrium was restored: this was in 8 minutes 6 seconds. The air was 50° , and the ether at first 50° ; but it rapidly sunk, as was found by dipping a very small bulb thermometer into it, to 28° . In a window with a moderate breeze.

2 and 3. Repeated the experiment in the same circumstances, except the evaporating vessel, which was now porcelain, and $2\frac{1}{2}$ inches diameter. Lost 40 grains in 3 minutes. Thermometer sunk from 50 to 30° . The two experiments made this way did not differ above one or two grains.

These results reduced to a vessel of $3\frac{1}{4}$ inches in diameter give

1st. Experiment, loss 17 grains per minute;

2 & 3 ————— — $22\frac{1}{2}$ —————

The reason why the result in the first experiment was something less than in the other two, was evidently owing to the circumstance of its longer duration, by which the ether was the greater part of the time in a low temperature, and consequently evaporated less. The ether used boiled at 102° . At 50° it was therefore in the capacity of water at 160° . But water at 160° , at most loses only 17 or 18 grains per minute and less 20° below that temperature. At first view therefore it should seem that ether evaporates quicker than the general law assigns. But it must be allowed that the temperature of the *air* has some effect upon evaporation, though it has certainly

ver

very little. Now ether in the above experiments is acted upon by a current of air of an equal or higher temperature than itself; but water of 160° is usually acted upon by air 100° lower than itself, which is every moment precipitating the vapour formed, and thus obstructing its circulation. This appears to be a sufficient cause for the small difference observed.

With respect to mercury, sulphuric acid, muriate of lime, &c. there can be no doubt but they experience a real evaporation like those above; but it must be very small in proportion as their boiling points are high. And it would be difficult to make experiments upon such of these as have an affinity for aqueous vapour; because their acquisition from the aqueous atmosphere would far exceed their loss by evaporation.

Since writing the above essay, opportunities have occurred to ascertain whether the evaporation from ice is conformable to the same law as that from water. Every one, who has tried the experiment, admits the fact that ice is evaporable. I have lately made several observations on this subject, the results of which, as far as they go, support the conclusion that the general law of evaporation continues the same below the point of congelation as above it. All the experiments were made in the tin vessel above described of six inches in diameter; a quantity of water was suffered to freeze in it, so as to form a circular cake of ice; the vessel and ice were then weighed together, and exposed in the open air for a certain time, after which being again weighed, the loss was found; the force of the aqueous atmosphere was sometimes determined during the experiment by a mixture of pounded ice and salt, in the manner already described.

Evaporation of mercury, &c.

The evaporation from ice agrees with the preceding law.

	Grs.	H.	Grs.	Wind.	Air.	
Nov. 5. In the night lost	110	in 9	; or, .20	per m.	N. E. brisk. 28° to 31°	Experimental
— at 10 A. M. —	25	in $1\frac{1}{2}$	; or, .33	—	N. E. mod.	32° results.
— 29. at 1 P. M. —	24	in $1\frac{1}{2}$	; or, .23	—	calm.	31°
— P. M. —	84	in $9\frac{1}{2}$	; or, .15	—	—	30°
— 30. in the night —	94	in 9	; or, .17	—	N. E. mod.	31°
Dec. 19. P. M. —	75	in 8	; or, .16	—	N. E. calm. 26° — 28°	
— In the night —	33	in 11	; or, .05	—	calm.	29°
— 20. A. M. —	21	in 2	; or, .175	—	W. mod	31°

Some of these being made in the night, and of long duration, neither the temperature of the air, nor the force of the aqueous atmosphere could be fairly determined: the second experiment

experiment was made under every favourable circumstance, and the aqueous atmosphere found at 22° . By problem ii, at page 13, it would have been determined at $21\frac{1}{2}^{\circ}$, using the second column of grains in the table.

On the subject of evaporation it may be considered as unpardonable not to advert to De Saussure's valuable Essays on Hygrometry.

Experiments of
De Saussure
compared with
the preceding
theory.

That excellent philosopher determined, by a well conceived experiment, that dry air of the temperature of 64° or 66° , imbibed aqueous vapour so as to increase its elasticity $\frac{1}{34}$ of the atmospheric pressure; and that a cubic foot of such air required 11 or 12 grains of water to produce the effect. By the table above at page 12 it appears the force of vapour at $61^{\circ} = .54 = \frac{1}{34}$ of 29.5 inches nearly. It is probable this difference is occasioned in part at least by the want of perfect dryness in the air he operated upon, which caused the increase of elasticity to be less than otherwise. It was, I think, unfortunate that he attached so much importance to and confidence in his hygrometer; and that he adopted the theory of chymical solution of water in air, contrary to the facts he discovered, which seemed more reconcileable to the notion of aqueous vapour being a distinct elastic fluid. Indeed he is forced to acknowledge in the 1st. chap. of his Essay on the Theory of Evaporation, that in the ordinary temperature of the atmosphere, aqueous vapour is formed in the first instance a distant elastic fluid, and *after it has been converted into an elastic fluid*, it is dissolved by the air; "*Je crois qu'il ne la dissout que lorsque l'action du feu l'a convertie en vapeur elastique.*" Now if it can for a moment exist independently under the pressure of the atmosphere, why may it not continue to exist in that state?

and on that ac-
count his tables
are inaccurate.

His table of the weight of aqueous vapour in a cubic foot of air at different degrees of the thermometer, being derived from experiments with his hygrometer, except the standard one of 60° (15° Reaumur), is far from accurate; and the inaccuracy increases with the distance from the standard, which, as has been observed, appears to be nearly correct; in the higher temperatures he makes the water dissolved too little, and in the lower temperatures too much. He says (§ 93) that the lowest he has seen the hygrometer in the open air, is 40; and that it indicated a reduction of temperature in the air amounting

amounting to $34^{\circ}.7$ (78° of Fahr.) was necessary in order to deposit dew. This observation alone is sufficient to render his hygrometer suspected; for, few who have attended to the formation of dew will admit the probability of so large a reduction being necessary in any climate or season: I believe it rarely requires 40° reduction in temperature in any part of the world to produce the effect.

Plate I.* is intended to illustrate the author's conception of the constitution of the atmosphere. The different marks or characters of the particles of the gases are merely arbitrary, and intended for distinction; the simple atmospheres are given nearly on their real densities, and the particles are arranged at equal distances from each other. In the compound atmosphere the same arrangement is made of each kind of particles as in the simple; but the particles of different kinds do not arrange at regular distances from each other; because it is supposed they do not repel each other.

Engraving to
shew the constitution of the
atmosphere.

III.

Observations on the Cultivation and Growth of Oak Timber. In a Letter from the Rev. RICHARD YATES, F. A. S. Chaplain to His Majesty's Royal Hospital at Chelsea, to CHARLES TAYLOR, Esq. Secretary to the Society of Arts†.

S I R,

TO expatiate upon the vast importance of increasing the growth of oak-timber, seems unnecessary. The national advantages resulting from this source appear to be in general well understood; and yet the cultivation and management of this most useful plant has not hitherto obtained that degree of attention which it most certainly merits.

Great importance of the culture of the oak.

Entirely to obviate, or even in some measure to remove or lessen, the obstacles that still continue to impede the planting

* From the same Vol. of the Manchester Memoirs.—I remind the reader of the references at page 257 of our last.

† For which the silver medal was voted. Memoirs for 1802, page 80.

of oaks, would therefore be rendering an essential service to the nation. The desire of accomplishing so beneficial a purpose, has induced the judicious and public-spirited conductors of the Society of Arts to propose a premium for "ascertaining the best method of raising oaks:"—in consequence of which, this paper is submitted to their candid consideration. And as the statements here made are founded upon a sedulous and active experience of fifty years, it is presumed the *spirit* and *meaning* of the Society's proposal may have been observed, although it has not been possible (in this instance) *literally* to fulfil its terms; at least, the very intention of promoting and forwarding the views of so enlightened and highly useful a Society, may, it is hoped, be accepted as an apology for calling their attention to these observations.

Cultivation of the oak discouraged,

It forms no part of the present design to enter minutely into the various causes that continue to operate in obstructing the cultivation of oak; as there is one of peculiar magnitude, the consequences of which are highly detrimental and injurious, and which it is therefore the principal object of this paper to remove.

from a notion of its very slow growth,

An opinion is generally prevalent, that the oak is particularly slow in its growth, and requires a great number of years before it affords any advantage. This idea too often deters from planting, on account of the very great length of time it is supposed the land must be occupied before any return of valuable produce can be obtained from it, after a considerable expence may have been incurred in forming plantations.

which is a mistake.

This opinion I consider as entirely founded in error, and to have taken its rise in a great measure from the want of proper management that has hitherto commonly prevailed in the raising of oaks: and in this paper I shall endeavour strongly to state, that the oak may be rendered very rapid in its growth, and that consequently land may be employed to great advantage in its cultivation, as a very considerable and profitable produce may, in a much shorter time than is generally supposed, be derived from proper parts of an estate thus employed.

Oaks are suffered to grow (slowly) with a short stem and large head: but their *size* may be

Oak-timber in this country, for the most part, appears in trees of a considerable extent of head, but seldom more than *twenty* or *thirty* feet in stem; and this, in many instances, the growth of a century. Now, by the course of management here

here proposed, it is conceived that trees, of at least *double* doubled in half
this magnitude, may be obtained in about half that time. *this magnitude*, may be obtained in about half that time:

It is not my intention to attempt a proof of this proposition by theoretical deductions, but to appeal for its confirmation to the indubitable test of fact, which, from the event of repeated trials, impresses a conviction, that experience will be found to support and establish it in the most unequivocal manner.

It would be easy to enlarge much on the various qualities of soil, the nature and process of vegetation, and the peculiar properties of the oak; but as these topics may be found amply and judiciously discussed in many other authors, who have expressly treated on these subjects, I shall decline all such speculations: and, with the hope of being more essentially useful, shall confine myself to a statement as simple and practical as possible.

The oak, in the progress of its growth, spreads numerous roots near the surface of the ground, and in an horizontal direction: these assist in supporting and preserving the tree in its position, but seem to contribute very little to its increase and magnitude. The oak appears to derive its chief nutriment and strength from a root that always descends at right angles to the horizon, and is called the tap-root. The first thing, therefore, to be observed is, that upon a judicious attention to this peculiarity, the planter's success principally depends; and the neglect of this care is the constant source of error and disappointment. In all climates, and upon all soils, to preserve this tap-root from injury, and as much as possible to assist its growth, is a general, and indeed the most essential principle in the cultivation of oak. With a due regard to this circumstance, the management of a plantation may be resolved into the three following practical directions:

Previously to planting the acorns, *loosen* the earth intended for their reception, by *deep trenching*. Instructions for
preparing the
ground and
planting acorns.

Never transplant, or in any way disturb, the saplings intended for timber.

Keep the plant carefully *pruned*, till arrived at a proper height.

More fully to elucidate the subject, and to prevent the possibility of misapprehension, it may be proper to give a more detailed statement.

Oaks for timber
are never to be
transplanted.

In determining on a spot to form a plantation of oaks for timber, it must always be recollected that the plants are to remain without removal in their first situation: the clearing and fencing may then be attended to as usual; and in the course of the winter, from September to March, the particular spots intended for the reception of acorns, may be prepared for that purpose, by digging a trench about three feet in width, and from three to six feet in depth, according to the closeness and tenacity of the soil. If grass-ground, the first spit should be placed at the bottom of the trench; and if more than one trench be necessary, they should be prepared in the same manner, preserving a distance of ten yards between each, if it be intended to employ the intermediate space in underwood, or for any other purpose.

Having made a careful selection of acorns that are perfectly sound, and in good preservation, they are to be planted about the middle of March. Draw a drill in the centre of the trench; two inches in depth, if the soil be heavy and loamy; but three inches in a light and sandy earth. In this place the acorns two inches asunder, and cover them carefully with mould. When the plants appear, they must be weeded by hand in the rows, and the earth of the trench round them cleaned with a hoe, once a month during the summer. In October inspect the rows, and thin them by pulling up every other plant: attention will of course be paid to remove the weak and crooked plants, and leave those that are tallest and straightest. On the second year, the operation of thinning must be repeated, at the same time, and in the same manner; and, should any of the remaining plants have made side-shoots stronger than the general character, they must be smoothly cut off with a sharp knife, close to the leading stem. On the third year, the thinning is again to be repeated, and the general pruning commenced, by cutting off close to the leading stem all the side-shoots of the first year; thus leaving the branches of two years to form the head of the following year. The removal of every alternate plant must be continued yearly, till the trees are about thirty feet apart, at which distance they may remain for timber. The pruning is to be continued, by removing every year, very smooth and close to the main stem, one year's growth of side branches, till the plants are arrived at a stem of forty, fifty,

or

or sixty feet, and they may then be permitted to run to head without further pruning.

Instructions for
cultivating the
oak.

The particular arrangement here recommended may be varied according to any peculiarities of situation, regard being constantly had to the general and most important principle of loosening the ground *very deep* previously to planting the acorns. By this mode of culture, oaks may be raised in almost any soil; but, where it is possible, a loam or marl is always to be chosen. Oaks thrive much the best in such earth; and, when assisted by *deep trenching* and *judicious pruning*, attain in a few years to an immense size.

Those who have been accustomed to notice the slow growth and stunted appearance of oak trees, when denied the assistance of art, and left to themselves in the common way, would observe with astonishment the vigorous and rapid increase of plants under the management now pointed out.

The plants thinned out the first three or four years, though not fit to be depended upon for timber, as transplanting generally injures very materially the future growth, may be replanted in the intermediate spaces between the rows, for the purpose of being afterwards removed; or they may be usefully placed in hedges, or other spare and unoccupied spots of ground. They should be headed down at the time of transplanting, as this operation assists the process of nature, in reproducing or remedying any injury the tap-root may have received from the removal: and, if proper attention be given to loosening the soil for their reception, and pruning them as they advance, in most instances an adequate profit will be derived from the labour bestowed upon them. After a few years, the produce of the timber-plantation will be found very advantageous. The young trees that are to be removed yearly, will always find a ready market for a variety of purposes unnecessary here to enumerate. In addition to these advantages, if by this treatment of *deep trenching* previous to planting, and *annual careful pruning* during the growth, timber can be produced in about fifty years, of equal quality, and much superior in size, to that which has been above one hundred years growing under improper management, or without the assistance of cultivation; it will doubtless be allowed that a most beneficial, if not absolutely the best possible method of "raising oaks," is here pointed out and ascertained.

This

Instructions for
cultivating the
oak.

This method of cultivation may perhaps be thought to occasion so much expence in manual labour as to prevent its being generally adopted: it might perhaps be sufficient to observe, that if the work be conducted with judgment and economy, the future produce would afford ample returns for all necessary expenditure: it should also be recollected, that the previous preparation of the ground, and the subsequent pruning of the plants, are both to be performed at that season of the year when a scarcity of work will enable the planter to obtain assistance upon easier terms; with this additional advantage also, of providing employment for the labourer at those times when the general state of agricultural business renders it difficult for him to find maintenance for himself and family without charitable relief.

In 1750, at Ingestrie in Staffordshire, the seat of Lord Chetwynd, some plantations were formed and managed in a great measure according to the principles here stated, and the growth of the plants were so uncommonly rapid, and so extraordinary, that it could not but attract the notice of all concerned in the conduct of them. The attention to the subject, then excited, has been the occasion and ground of all the observations and experiments made from that time to the present, the result of which is given in this paper.

The extensive plantations of the late Lord Denbigh, at Newnham Paddox, in Warwickshire, are well known and much admired. The whole has been conducted with great judgment. About a square acre has been employed in raising oaks upon a plan nearly similar to that now proposed, and affords the best and most convincing proof of the superior utility and efficacy of such management. Had the noble Earl been now living, I should have been enabled to have laid before the Society some more detailed particulars; That, however, is now impossible; this paper, therefore, in its present state, may perhaps be thought not altogether unworthy of notice, as tending to forward the liberal designs of the Society, and contributing to the advantage of the public, the author conceiving that the best method of raising oaks is ascertained and stated in it.

Should the Society be in any degree inclined to join in this sentiment, it may perhaps induce them to make some alteration in the terms of their proposal; as, according to the statements
made

made in this paper, and indeed from what may be seen in every part of the kingdom, in the character and appearance of oaks growing without cultivation, it seems ascertained, that "acorns set with the spade or dibble, without digging or tillage," can never be depended on to form good timber; and even in the most favourable circumstances of this case, the growth will be exceedingly slow and precarious. The same may be said of "young plants, previously raised in nurseries, and transplanted;" for if the tap-root be cut, broken, or in any degree injured, which in transplanting it is almost impossible to avoid, that plant will seldom become a vigorous and flourishing tree. To form a course of experiments on such a plant as the oak, is not a very easy matter. To fulfil explicitly the conditions of the Society would require a great length of time, and would be attended with considerable expense, from which future candidates may in a great measure be exonerated. The raising even one acre in the manner here ascertained might be productive of great pecuniary advantage, if the facts and experience detailed in this paper are permitted to prove the inutility of the other two methods, and consequently to remove the necessity of employing so much ground upon them, at an expense they will never repay.

Instructions for
cultivating the
oak.

Chelsea College, Nov. 4, 1801.

IV.

A First Memoir on coloured Shadows.

By Cit. J. H. HASSENFRATZ.

(Concluded from Vol. VI. page 285.)

PART THE SECOND.

THOUGH the shadow produced by the concurrence of the solar light with that of the atmosphere, usually presents only that series of colours contained in the portion of the spectrum, between green and violet, it is not the same with the coloured shadows observed in the interior of rooms, when several lights direct or reflected concur with that of the atmosphere to enlighten the plane on which the shadow is observed

Shadows produced by more than two lights, are more various in their tints than those before described.

observed; these are capable of shewing all the colours of the prism; in fact, we see red, orange, yellow, green, blue, indigo and violet, more or less blackened.

Number of shadows.

The number of these shadows is almost always two or three, sometimes four or five; it has even happened that we have distinguished six.

When these shadows are two in number, the colours they present are always those called *complementary* colours.

Combination of colours to imitate the solar spectrum.

The infinite variety of colours observed in the solar spectrum may be imperfectly imitated by combining different proportions of the natural colours, red, yellow and blue; and, according to the observations of Newton, an artificial white may be formed, by mixing these three colours. Those colours are denominated *complementary*, which must be made with one or two of the three colours cited, to produce an artificial white with a given colour.

Complementary colours.

To obtain an artificial white with a red, the yellow and blue must be mixed with it. The yellow and blue make a green, green is therefore the complementary colour of red, and *vice versa*.

The complementary colour of the orange formed by yellow and red is the blue.

The complementary colour of yellow is violet.

The term does not subvert the Newtonian theory of the solar light.

As the complementary colours meet together in a great number of cases, we have thought it necessary in the outset to give them that denomination, which makes no change in the results, by which Newton proves that the solar ray is composed of an infinity of homogeneous coloured rays.

If the spectrum is divided into two parts at the point where the green commences, all the homogeneous rays of the lower part, that is to say, from the beginning of the red to the beginning of the green, are complementary to those of the upper part, that is to say, from the green to the violet; and that in the natural order of their appearance.

The denomination of complementary colours has no other object but to point out two colours which are found together very frequently; and to avoid, by this simple medium, the circumlocution which their distinction would lead to.

Complementary colours of two shadows;

Whenever two coloured shadows are seen, they are nearly always complementary to each other; that is to say, if the shade of one is red, orange, or yellow, that of the other is green, blue, or violet.

When the number of the shadows is three, one of them is of three; almost always complementary to the other two.

When there are four shadows, two of them are sometimes of four, complementary to the other two; at other times, one of the shadows is complementary to the remaining three.

When there are only two coloured shadows, one of them is always of the colour of one of the bodies which reflects the light. If the apartment, in which the shadows are seen, is commanded by a covering of slates which reflects a bluish light on the plane where the shadows fall, the two colours are blue and orange; if the covering is of tiles, the colours are nearly the same. If a meadow or trees reflect light into the apartment, the shadows are green and red. If the apartment has coloured hangings which reflect the atmospheric light or that of the sun, the shadows will participate of the colours of the lights reflected externally and internally.

One of the shadows is of the same colour as the reflected light;
which may be varied by another light,

When the light is reflected through glazed windows upon the plane where the shadow is seen, as the glass is rarely colourless, and generally tinged with green or violet, according as iron or manganese predominates in its composition, the shadow participates in the colour communicated to the light in its passage through the glass.

or by passing through glass,

If the surface on which the different lights are received be itself coloured, the colours of the shadow will be affected by it, and the problem will become more complicated.

or by the colour of the surface on which the shadow is received.

From the observations we have collected, it follows naturally, that the coloured shadows of the interior of an apartment are capable of yielding all the colours of the spectrum; that these colours are governed by those of the bodies which reflect the light and illuminate the surface on which the coloured shadows are observed. Thus far these observations offer nothing which could not have been foreseen by every person who had reflected a little on shadows; but that which is remarkable is, that these shadows are almost always accompanied by other shadows whose colours are complementary to the reflected colours, although no body is discovered which is capable of reflecting these sorts of colours.

Inferences.
Other complementary shadows not produced by direct reflection.

Here then is a new shadow which seems constantly to accompany the first, whose colour has a peculiar affinity for that of the light which produces the primitive shadow, and depends on causes which we purpose to examine in a subsequent memoir.

Part the Third.

Shadows from
two lights are of
different colours,

IF in a clear moon-light night, at a time when the lamps are alight in Paris, an opaque black body be brought near a white paper, enlightened both by the moon and a lamp, two distinct coloured shadows will be perceived, one reddish, the other bluish.

If a taper, a candle, a lamp, or any other light, be brought near a white pasteboard enlightened by the moon, an opaque body placed at a small distance from the pasteboard, forms two coloured shadows; that occasioned by intercepting the light of the moon is reddish, and that produced by the other light is blue.

These two shadows may be obtained by enlightening a surface with the atmospheric light and that of a lamp; but this requires that the light of the atmosphere must enter the room where the experiment is made, through a small opening, in order that the shadow may be well defined when this light is single.

The effect was
not varied by
the nature of one
combustible
body made use
of.

By admitting the light of the atmosphere into a darkened room through an opening of a decimetre (four inches) in diameter, and illuminating a white pasteboard by this light and that of a lamp, we have found that when the pasteboard was one or two metres (yards) from the opening, two coloured shadows were produced; that of the atmosphere is constantly red, and that of the artificial light, blue. We have employed the lights from coals, wood, tapers, candles, alcohol, and even hydrogen gas.

The greatest part of the shadows obtained from two different lights are of two tints, the one reddish, the other bluish.

The variations in the colour of shadows are independent of the intensity of the lights which illuminate the surface. We were careful in all the experiments we are going to relate, to place the light at such distances that they should enlighten equally that part of the white pasteboard on which the shadows were projected; and to this end we followed the method made use of by Bouguer, and which consists in receiving through two holes, on a piece of oiled paper, the rays from two different lights, and removing the strongest, or bringing forward the weakest, until the two lights were of equal intensity; always being careful to place the eye at an equal distance from the two enlightened points.

The

The experiments were made in a dark room, every part of which was painted black, for the purpose of destroying the colour produced by reflected light. Precautions in making the experiments.

We at first illuminated the pasteboard by the light obtained from the combustion of fish-oil in a common lamp, and by the light of hydrogen gas, produced by the dissolution of zinc in sulphuric acid weakened with water. The shadow produced by intercepting the light of the hydrogen gas, was reddish; that arising from intercepting the light of the lamp, was bluish. Shadows from hydrogen gas and oil;

At the beginning we only used a feeble light from the hydrogen gas. Being fearful that the colour of the shadows might be occasioned by the weak light, we filled a bladder with the gas, and by compressing the bladder strongly, produced a powerful bright light: the plane illuminated by this light and that of the lamp, afforded a similar result; the shadow from the hydrogen gas was reddish, that from the lamp bluish.

On enlightening the pasteboard by the light of a lamp and that of alcohol, the interception of the light of alcohol produced a reddish shadow, and that of the lamp a bluish shadow. from a lamp and alcohol;

By illuminating it with the light of the lamp and that of a candle, the shadow from the candle was black, with a light-reddish tinge; that of the lamp was black, with a light-bluish tinge. from a lamp and a candle;

The pasteboard being illuminated with the light of a lamp and that of a taper, the shadow from the taper was black slightly reddened, and that of the lamp, black a little bluish. from a lamp and a taper;

The pasteboard illuminated, at the same time, by the light of a lamp and that from the combustion of wood, the shadow thrown by the light of the wood was a violaceous blue; that of the lamp a reddish yellow. from a lamp and wood,

The experiment was tried with wood burning with difficulty, and producing little flame; with very dry wood producing much flame; with chips of wood producing a white flame, which reddened that of the lamp. When the surface was enlightened with lights of equal intensity, the coloured shadows offered little variation; that proceeding from the light of the wood was violaceous blue, that of the lamp reddish yellow. in different states.

The first experiment was repeated in glass-houses where wood is burnt. However bright the flame might be which was emitted through the working flues, the shadow produced by it on a surface also illuminated by the light of a lamp, was a violaceous blue, while that from the lamp was reddish yellow. The experiment repeated in a glass-house where wood is burnt;

On

in an iron forge; On illuminating the pasteboard by the light of a lamp and that from the combustion of culm in an iron forge, the shadow arising from the light of the culm was bluish, and that from the lamp reddish.

in a glass-house where culm is burnt; On illuminating the pasteboard by the light of a lamp and that from the culm burnt in the furnaces of a glass-house, where the brilliant flame of the working flues reddened that of a lamp, the result was similar to the preceding: the light from the culm produced a blue shadow, and that of the lamp a reddish shadow.

with a lamp and charcoal. When the pasteboard is illuminated by the light of a lamp and that from the combustion of charcoal, the shadow of the charcoal is blue, and that of the lamp red.

The pasteboard illuminated by the light of a lamp and that of charcoal urged by the large bellows in a chemical furnace, so as to produce a whitish light as well as that violaceous light sometimes observed in the furnaces of a forge and in tall furnaces, constantly afforded the same result. In these two cases the shadow from the light of the charcoal was bluish, and that from the lamp reddish. The identity of the colours of the shadows obtained in these three experiments with charcoal, notwithstanding the colour of the flame was different in each experiment, leads to an opinion that the result depends on the nature of the combustible. Nevertheless, to obtain more probability, we made further observations. We filled a chemical furnace with charcoal, and having covered it with its dome for the purpose of comparing the shadow of the direct light of the violaceous flame arising from the combustion of the charcoal with that of the light of a lamp, we took notice of the shadows of these two lights at the moment when the flame appeared at the summit of the furnace. For a considerable time we observed that the colours of the shadows were very uncertain: sometimes the shadow of the lamp was blue, and that of the flame red; at other times the shadow of the flame was blue, and that of the lamp red. This alternate change in the colours of the shadows from blue to red, continued until the charcoal, which was in the upper part of the dome, was completely inflamed; then the colours of the two shadows became fixed, that from the flame of the charcoal became bluish, and that from the light of the lamp reddish.

Probability that the colour of the shadows is produced by the nature of the combustible. Singular change of colour in the shadows. Repetition of the experiment by burning the

As, from the observations of several persons to whom these results were communicated, we were induced to believe that the

the great intensity of the light of charcoal obtained by its combustion in oxygen gas, might change or make some variation in the colour of the shadows, we ignited the extremity of a piece of charcoal, and urged its combustion by a jet of oxygen gas; the gas was contained in a bladder, which was pressed by the arm; the light obtained was extremely brilliant. A white parchment, illuminated at the same time by this light and that of a lamp, presented two coloured shadows; that from the light of the charcoal was blue, and that from the lamp red.

charcoal in oxygen gas,

with a similar result.

From the experiments we have detailed it results, that the shadow of the light of hydrogen and of alcohol is reddish, when that of the lamp is blue; that the shadow of the light of wood, of culm, of charcoal, is bluish, and that from the light of the lamp is reddish; and as hydrogen and alcohol contain less carbon than oil—and dry wood, culm, and charcoal contain less hydrogen than oil; it would seem that the blue and red colours of the shadows of the two artificial lights, bear a relation to each other corresponding with the proportions of these two combustibles; that the light produced by a substance in which hydrogen predominates, gives a reddish shadow, and that produced by a substance in which carbon predominates, affords a bluish shadow.

Summary of the experiments;

whence it is inferred that the colour of the shadows depends on the predominancy of hydrogen or carbon.

Although our experiments appear to lead to that conclusion, and we took great care to be certain that the red and blue shadows produced by the two lights were independent of the rapidity of the combustion and intensity of the lights; nevertheless, we dare not hazard the presenting this result as forming a general law; we prefer waiting until time and new experiments shall confirm or destroy them.

Recapitulation.

FROM the observations and experiments related in this memoir, it follows,

1st. That the shadows formed by the direct light of the sun and that of the atmosphere, vary from meadow-green to a violet-black, in a gradation through the blue, indigo, and violet; and that this variation depends on the intensity of the light (of the sun) compared to that of the atmosphere.

2^d. That the shadows formed in apartments by the light of the atmosphere and reflected lights, may present all the prismatic

matic colours, more or less changed by black; and that the tints of the shadows observed are always complementary to each other.

3d. That the shadows produced on a pasteboard illuminated by artificial lights, are reddish and bluish, more or less deep, and that very probably the bluish and reddish tints of the shadows depend on the proportions of hidrogen and carbon in the combustible bodies.

The subject to be resumed.

We purpose, if the Institute judges this subject worthy of their attention, to make known, in other memoirs, the experiments we have made to determine the different causes which contribute to form coloured shadows in every particular circumstance.

V.

Account of the Preparation of the Hyacinthus non scriptus, or common Blue-Bell, as a Substitute for Gum-Arabic. In a Letter from Mr. Thomas Willis of Lime-Street, London, communicated to Charles Taylor Esq. Secretary to the Society of Arts.*

SIR,

Communication by Mr. Deyeux respecting gum from the hyacinthus non scriptus.

I HAVE observed in the Appendix of the last Monthly Review, in their Report of the Chemical Annals, No. 115, that mention is made of a letter from Mr. Deyeux, to the authors of the Annales de Chimie; in which an account is given that a gummy substance had been discovered, by Mons. Deyeux, to be contained in the bulb of the plant called hyacinthus non scriptus; and that the reviewers only say, "This article does not at present require any further notice than annunciation." As I know, Sir, your benevolent disposition in promoting and encouraging the arts, the sciences, and manufactures of this kingdom, I beg leave to offer the following observations on the above-mentioned article, which I think a subject proper to lay before your most excellent Society, and which I have no doubt may become of national utility.

* Extracted from the Society's Memoirs from page 202: The silver medal was adjudged to this Communication.

In the year, 1794, whilst collecting plants in a wood for botanical specimens, I observed that the root of the *hyacinthus non scriptus*, the plant commonly called blue-bells, or hare-bells, was extremely mucilaginous; and on tasting it, I discovered only a very slight pungency. I collected a pound of the bulbs, and, after slicing and drying them before a fire, they yielded about four ounces of powder. I thought that by keeping the powder some time, the little acridness might go off, as it does in the arum-root powder. I tasted it about six months after, and found it perfectly insipid. I concluded it might be rendered useful for food or nourishment, but at that time pursued the matter no further.

Discovery of this
mucilage by the
author in 1794

In the spring of 1800, gum-arabic having been a long time very dear, and likely to continue so, I thought this mucilaginous root might answer some of its purposes, for external use. I therefore procured seven pounds and a half of the bulbs, which, when sliced and dried, produced two pounds of powder. Being soon afterwards in company with Mr. Charles Taylor, Secretary to the Society of Arts, &c. I mentioned to him that I had discovered a root which grew in great plenty in this kingdom, yielded a very strong mucilage, and which I imagined would answer the purpose of gum-arabic, in some of the manufactories. He said, if I pleased, he would send some of it down to Manchester, to be tried by the calico-printers.

Proposal in 1800
to use it instead
of gum-arabic.

Three or four ounces of the powder were given him, and sent down there: he was informed, upon trial, that it answered the purposes of fixing the calico-printers colours, equally as well as gum-arabic; and in the same proportion, of an ounce and a half of the powder, to four ounces of the mordant. Mr. Taylor received the samples of the printed cottons on which it had been used.

Trial at Man-
chester success-
ful.

On the 15th of January, 1801, I furnished Mr. Taylor with eight ounces more of the powder; but have not since heard the result.

As this root can be easily procured, and used at a less price than gum-arabic has been sold for several years past, I think it may be rendered of great utility; and the Society of Arts, &c. by patronizing it, may be the means of making it a public benefit.

The root is
cheap.

Care

Instructions
for collecting
the roots and
securing their
growth.

Care should be taken, and advice given, that the woods should not be left destitute of the roots; and it would be advisable to offer premiums for the cultivating the roots and offsets, as they are very increasing. By such means a constant supply may be had, if the roots answer the intended purposes.

I do not presume to offer any thing respecting the mode in which the Society may think proper to divulge the discovery, and promote the use of these roots; but I imagine, that if the roots are bruised and used fresh, they would answer the purpose better than when dried and powdered; and as it is now a proper time of the year for taking them up, and will continue to be so for two months, I wish that the discovery may be made known as soon as possible.

I have sent you specimens both of the dried roots and powder, that they may be seen at the Society's rooms, by the calico-printers. What I have done have been scorched a little in drying; but the colour would be much better, if proper care was taken in drying them.

I am, Sir,

Your most obedient Servant,

*Lime-Street,
March 17, 1802.*

THOMAS WILLIS.

*To John Baker, Esq. one of the Mem-
bers of the Society of Arts, &c.*

From the trials made before the committee with this powder, with hot and cold water, from samples of the printed cotton produced which had been printed therewith instead of gum senegal, and from experiments made in Manchester, it appears that the hycinthus non scriptus may, in many cases, be found a useful substitute for gum-arabic.

VI.

A Memoir on Vinous Fermentation: by Cit. THENARD.*

THE vinous fermentation has hitherto received more attention than the acetous or putrid, not perhaps that it has any thing in it more remarkable, or more worthy of our consideration, but because it is in the natural order of things, to take more interest and set more value on what is of most immediate utility.

The vinous fermentation most attended to because most useful.

The date of the discovery of the vinous fermentation appears too well established to be called in question. All historians agree in saying, that the most ancient nations knew how to prepare spirituous drinks. It ascends therefore to the remotest times; and, if we pay any credit to the poets, we must carry it back to the fabulous ages. Indeed it would be surprising, if it escaped the notice of the earliest of men. An ebullition arising spontaneously in a liquid, a whole mass rising of itself, a sweet liquor becoming vinous, the change of a saccharine matter into an ardent spirit, are all extraordinary things, calculated to strike the attention, and awaken the desire of tracing them to their first causes. Accordingly there is no phenomenon more early observed, and none that has been the subject of more consideration, or has given birth to more experiments; yet, from one of those contrasts that rarely occur in the annals of science, though it has been the most studied, there is not perhaps one, with which we are less acquainted. It has been a rock, on which the endeavours of chemists in all ages have split. Becher, so celebrated for his subterranean physics, Stahl, the Nestor of the ancient chemistry, Boerhaave, whose ideas were so great, Rouelle, to whom science is indebted for part of the progress it has made during the last half century, and Macquer, that master in the art of writing, all failed in their attempts to penetrate this mystery of nature. Lavoisier, who was capable of surmounting the greatest obstacles, is the only person, who, enlightening the whole sphere of chemistry by his genius, travelled this obscure path without wandering out of his road. His inquiries into fermentation will ever remain a model of vegetable analysis. In this, as in every thing else

Known to the most ancient nations.

Its phenomena excite attention,

and have led to many experiments;

hitherto little known.

All chemists have failed to explain it,

except Lavoisier who alone made some progress in it.

His investigation is a model of vegetable analysis.

* *Annals de Chimie*, June 1803. p. 294.

But he left it imperfect.

he did, he observed that strictness of deduction and accuracy of operation, which are his characteristics, and which may be considered as the source of the splendid discoveries, that will for ever illustrate his name. Still, notwithstanding the excellence of what he did, he was far from leaving nothing more to be done. Though he shed great light on this part of science, the obscurity with which it was enveloped was so great, that it is still seen only through a mist. This is a truth that did not escape himself; he was well aware that he had only laid open the path to the goal, which no doubt he would have reached, no doubt he would have completed the career he had begun with such success, had not death, jealous of his fortune and glory, robbed science of his labours.

In fermentation sugar is converted into alcohol and carbonic acid, by means of an intermediate substance. But what is this? and how does it act? Imagined to be extractive matter, mucilage, tartar, which last is not found in all fermenting liquors,

or a mixture of all these.

All we know of fermentation in fact is confined to this, that the saccharine matter is converted into alcohol and carbonic acid by means of an intermediate substance. But what is the nature of this substance? and how does it act on the sugar? These two grand questions form the subject of the present memoir; questions that have been often attempted, without ever having been solved. Some have thought, that the fermenting principle resided in the extractive matter. Others would have it to be in the mucilage, because this more frequently accompanies the extractive matter. Some, again deceived by the presence of tartar in wine, have imagined they found in this the true ferment; but if, instead of confining themselves to the fermentation of the must of grapes, they had turned their attention to that of other juices, in which analysis cannot discover the existence of this salt, they certainly would not have fallen into this error. Others, lastly, inconsiderately adopting all these opinions, have asserted, that a mixture of these different matters preside as it were over fermentation, effecting the decomposition of the sugar, and its conversion into alcohol*.

Of these hypothesis some are evidently false; others are seducing, and acquire from specious reasoning some degree of probability. But before we admit them, we must consult ex-

* Mr. Thenard here gives a long note, to prove he had not seen the prize essays written in 1787 and 1788 by Fabroni, from whom some persons at Paris accused him of having borrowed, and to show, that Fabroni and he differ essentially in this theory; but it is here omitted as irrelevant. T.

perience;

perience; we ought, leaving them all out of the question, to deduce from observation the theory, which we are too apt to form beforehand. If the genius of Stahl, instead of giving birth to phlogiston, a being that never existed but in the brilliant imagination of that extraordinary man, had inter-^{Stahl misled by hypothesis and neglect of experiment.}rogated nature by means of experiment more than he did, perhaps it would not have gone astray; perhaps Stahl would have discovered the truth, and deprived France of the glory of having produced the author of the modern theory of chemistry. Such is the course I have pursued. Before I formed or adopted any system, I observed facts, and deduced from them consequences, which, it appeared to me, must guide us to the view of what passes in liquors under fermentation. But in a subject of such nicety nothing is more easy than to deceive ourselves; and it is particularly for the purpose of correcting my notions, if they be not just, that I submit to the class the result of my inquiries*.

Facts observed by the author.

My first observations were made on the juice of gooseberries, which I had strong reasons to prefer to any other; its fermentation proceeding with most celerity, so that it is consequently best calculated to throw light on the causes that produce it. ^{He first examined gooseberry juice, as fermenting most quickly;} All my researches were directed at first to discover the matter, in order to discover what serves as the ferment, that serves as a ferment. It would be making a great step, and almost resolving the problem, or at least discovering a number of truths not yet known, to determine the nature of this matter, and to ascertain whether it be always one and the same, or whether there be several that possess this property. ^{and whether it be one peculiar substance, or several.} This important question had struck me long ago: I had even meditated upon it occasionally, and promised myself to attempt its solution, when the Institute proposed it as a subject of a prize. This was an additional strong motive to my pursuing it. I was far from being disposed to admit several fermentative principles; every thing led me to believe, that there was but one, and that it was none of those hitherto suspected, since ^{He supposed it to be one and the same;} in fact neither extractive matter, nor mucilage, nor tartar, &c. acts upon sugar. But this required positive demonstration; and though I have yet no absolute proofs of it, as it is by no means demonstrated, that there are several, and one is observable. ^{and this, though not demonstrated, most probable.} served on all occasions, this opinion seems to me at present to deserve the preference.

* This paper was read to the National Institute.

Gooseberry juice contains a slightly glutinous matter, which, separated by filtration,

occasioned a mixture of sugar and water to ferment,

and become vinous.

But as it scarcely effects this on six times its weight; it may be presumed to contain but a small portion of the fermentative principle.

This could not be obtained apart; not apparently altered by having produced fermentation; but it ceased to yield ammonia.

The germ of fermentation is therefore of an animal nature,

not separable by reagents,

The author therefore had recourse to fermentation.

Through a linen cloth of close texture I pressed out the juice of a killogramme of gooseberries. It was turbid, and held in suspension a slightly glutinous matter, which I separated by the filter, and washed in a large quantity of water. As nothing is to be neglected in experimental science, and the most trifling fact frequently leads to important consequences, I subjected this matter to a regular examination. I first mixed it with sugar and water, to see whether it would cause them to ferment; and I soon perceived many bubbles of an elastic fluid to be extricated, which I found to be carbonic acid. The effervescence continued a week, and at the end of this period the liquor was a pleasant drink, but slightly saccharine; it contained a great deal of alcohol, and might easily have been mistaken for a wine not yet completely made. It may be supposed I redoubled my zeal and attention in the examination of a substance, that offered me what I sought. It was natural first to inquire, whether the whole of it were adapted to decompose sugar; but a sixth part of its weight being scarcely able to effect this decomposition, I concluded, that it contained the fermentative principle only in small quantity. This I attempted every method in vain to separate, and obtain apart: nothing therefore remained for me, but to compare it before and after it had served to produce fermentation. It was not apparently altered by this process; being still insipid, insoluble both in water and in alcohol, and affecting neither infusion of litmus nor syrup of violets: but on distillation it no longer afforded any trace of volatile alkali. This result, at which I was not surprised, and which a second experiment confirmed, was nevertheless a ray of light, that confirmed me in the course I should pursue. It showed me, that the germ of fermentation was of an animal nature, it agreed with the ideas I had before conceived, and gave to my suspicions an appearance of reality.

I now examined the juice of gooseberries with great care, to discover this animal matter, which I considered already as the true ferment. As it was insoluble by itself, it must be combined with some substance, that held it in solution. All the reagents I employed failing to answer the purpose, I had recourse to fermentation itself, and observed the phenomena produced by it under all circumstances. I made my experiments on nearly a litre of filtered and perfectly clear juice.

The apparatus was placed in a stove, where the thermometer stood at 20° : it was not long before a fermentation was evident; a large quantity of carbonic acid was presently evolved; much froth was formed; the liquor lost its transparency, and it even became so turbid, that a sediment was thrown down, which was more evident as the fermentation approached its end. This sediment was of a yellowish white colour, glutinous, void of taste, grew brown on drying in the open air, and became slightly acid. Thrown on red-hot coals it burnt in the same manner as animal substances: distilled in a small retort it afforded a considerable quantity of carbonate of ammonia even crystallized. It made sugar ferment with extreme promptitude. In short it was a substance perfectly analogous to the yeast of beer.

Filtered gooseberry juice, in a heat of 20° soon fermented; evolved much carbonic acid, was covered with froth, became turbid, and deposited a sediment of a yellowish white, glutinous, insipid, grew brown on drying, and slightly acid. On red-hot coals burnt like an animal substance, and afforded carbonate of ammonia when distilled. It caused sugar to ferment; and is perfectly analogous to yeast. This substance common to all fermenting juices. Grape juice yields most next to that of gooseberries. The juices of pears and apples afford little, whence they ferment slowly. Yeast commonly formed where alcohol is produced; even from honey and water, and diabetic urine.

I was eager to try whether this phenomenon were general, as it ought to be according to my mode of reasoning: and in fact experience soon taught me, that it was common to all juice in a state of fermentation. The must of grapes, the juice of cherries, pears, peaches, and apples, and the decoction of barley and of wheat, afford yeast in their fermentation. The grape juice yielded more than the others, though less than that of gooseberries: accordingly it did not ferment so readily as the latter. The juices of cherries and peaches deposited nearly the same quantity: those of pears and apples afforded very little, which is the reason why their fermentation is so slow. I could have wished to have had a greater number of fruits at my disposal, that I might have varied my experiments more: they were sufficient, however, to prove, that, where alcohol is formed, a sediment of yeast is commonly formed likewise. If they who have any doubt on the subject remaining will maturely consider the two following experiments, I believe they will find themselves convinced. I knew that honey diluted with water would gradually be converted into a liquor containing spirits. Cullen informs us, that the saccharine urine of a diabetic patient undergoes in time the same change*. Accordingly I set both of these to ferment, and the sediment of yeast was formed in each.

* It was Dr. Dobson of Liverpool, who first found, that diabetic urine changed first into a vinous liquor, and afterward into an acetous, before it became putrid. T.

So that in every spirituous fermentation an animal matter is deposited similar to yeast.

Is this generated in the process of fermentation ?

No direct proof, that nature employs it exclusively as a ferment.

If it be a product of fermentation, it probably comes from some particular substance, little changed.

Yeast an immediate principle of vegetables.

and if there be any other ferment, it must be similar to it, and composed of azote, oxygen, carbone, and hidrogen.

It may be laid down, therefore, as demonstrated, that in every spirituous fermentation an animal matter is deposited, similar in all respects to that arising from wort, possessing absolutely the same properties, and in particular that of decomposing sugar, and converting it into carbonic acid and spirit of wine. This gives rise to a new question, that naturally presents itself, and ought next to occupy our attention. Is the yeast generated in the process of fermentation, or rather was it already formed, and did serve as a ferment ?

I must confess we have yet no experiments, which directly prove, that nature employs this substance exclusively to effect the conversion of sugar into alcohol and carbonic acid. For why should it be deposited when the fermentation has taken place ? It may be said indeed, that the sugar holds it in solution, that it can dissolve more than is necessary for its decomposition, and that then the excess is precipitated. But this theory is feebly supported by experiment. Is this a sufficient reason, however, to reject it altogether ? Have we not several instances of compounds, that require much time for their formation ? and this perhaps is what occurs in the juices of fruits, where the ferment and the sugar are long in contact with each other. What is certain, or what at least appears probable, is, that, if yeast be a product of fermentation, as all liquors that ferment deposit it, no doubt it owes its origin to one and the same soluble substance, from which probably it differs little, and which produces it by its reaction on the sugar.

Whichever of these two opinions obtains the preference, after more numerous trials, as I have no doubt, that yeast is an immediate principle of vegetables, and in consequence acts an important part in the phenomena both of art and of nature ; as I am likewise persuaded, that, if any other substance capable of exciting fermentation exist, it is in the highest degree analogous to yeast ; that it differs from it very little ; that it is composed like it of azote, oxygen, carbone, and hidrogen ; and lastly that it has no doubt the same mode of acting on sugar : I shall proceed to exhibit with the greatest care the properties of this matter, which I shall henceforward term ferment, and in particular consider well its action on the saccharine principle, in order to establish the theory of fermentation. This theory, even supposing yeast not to exist already formed, but to be produced in fermenting juices, will still be of utility, and capable of various applications, as will appear below.

I shall

I shall not recapitulate the physical qualities of ferment, as I have mentioned them several times already; but I shall confine myself to its chemical properties, which alone are of essential import. It has no taste. It neither reddens infusion of litmus, nor changes syrup of violets green. The putrid fermentation, which in time it undergoes, is in every respect similar to that of animal substances. By desiccation it loses three fourths of its weight, and this loss consists entirely of water. Thus dried it is still capable of exciting fermentation; it is by no means decomposed, and may be preserved in this state without alteration for an indefinite time. We may avail ourselves of this property, to convey it to places at a distance from any brewery, or with which it is so difficult to keep up an intercourse, that fresh yeast could not be sent to them, particularly in summer, without becoming putrid. Distilled in a small retort, and urging the fire so far as this would bear, eight parts of ferment left a residuum of 2.83 of coal; and I obtained 1.61 of water, 1.31 of oil, and 1.46 of muriate of ammonia on adding muriatic acid. Finally, I collected 0.33 of gas, containing a fifth of its bulk of carbonic acid, and which, when this was separated from it by potash, burnt like carbonated hydrogen, and required 1.5 of oxygen for its combustion. From this experiment we see, that ferment contains in particular a great quantity of carbon.

Chemical properties of ferment.

May be preserved any length of time when dried.

Analysis.

Water at the temperature of 12° or 15° does not dissolve $\frac{1}{100}$ of ferment: indeed it dissolves so little, that after standing upon it several hours, and being well filtered, it has scarcely any action upon sugar. Boiling water occasions it to undergo a decomposition, which I shall examine in another memoir.

Insoluble in water.

Boiling water decomposes it.

Nitric acid, even diluted with water, at eighteen degrees, decomposes it also: it converts it into grease; and there is evolved from it at first azot mingled with carbonic acid, and afterward nitrous gas at the same time.

Nitric acid converts it into fatty matter; and azot with carbonic acid, and nitrous gas are given out. With potash it forms soap, and ammonia is extricated.

Potash acts upon this substance in the same manner as upon animal matters, and the phenomena in both cases are perfectly the same: with each it forms a saponaceous substance, and a great quantity of ammonia, that is volatilized.

Its action upon sugar.

But of all the properties of ferment, no one is so remarkable, and at the same time so useful, consequently no one so much deserves to be studied, as its action upon sugar: this is interesting to men in every class of society, from the mechanic to the

the philosopher; to both by its products, and to the latter because it may be a fertile source of reflection and of new truths. It is much to be regretted, that Lavoisier did not pursue the investigation as he intended, and examine it with that care, which is conspicuous in all his labours. Who was more capable of giving birth to a theory of fermentation, than the author of the modern theory of chemistry? No doubt he was prevented from doing this by a concurrence of circumstances: and this theory, important as it is to science, has hitherto remained vague and hypothetical. Knowing the fermentative principle, it could not avoid naturally making a part of my researches; if I have not rendered it as clear as I hoped, at least the veil with which it was covered is removed, and it rests on reasoning confirmed by experiment.

To obtain the solution of this problem, I added together different quantities of ferment and sugar; I observed in every case what became of both; and I confirmed by farther observations what the preceding had suggested. Sixty grammes (927 grains or nearly one ounce of ferment) not dried, and three hundred grammes (4630 grains) of sugar entered into fermentation readily, the temperature being 15° (centigrade=59° Fahrenheit.) In four or five days all the saccharine matter had disappeared; 51.5 litres (3041 cubic inches) of carbonic acid had been evolved; the liquor being filtered, and distilled to two thirds, gave on a second rectification 863 grammes of spirit at 13°. The apparatus was so contrived, that nothing was lost: the receivers were cooled with common salt and ice. I found by synthesis, that this quantity of spirit was equivalent to 171.5 grammes of alcohol at 39°. The residues left after distilling the spirit were poured into dishes and evaporated to dryness: from the residuum of the second distillation nothing was obtained, but that of the first yielded about 12 grammes of a nauseous substance, slightly acid, and feebly attracting the moisture of the air. I wished to discover the nature of this acid, but there was too little to ascertain it. Lavoisier says, it is the acetous. Lastly, of the sixty grammes of ferment, there remained forty grammes of a substance, which I believed to be more animalized than the ferment itself. I was much surprised to find, that I obtained from it by distillation much less ammonia. Hence I suspected, that by mixing it afresh with sugar, fermentation would again take place, and thus

Of the precipitate one part with five of sugar fermented readily.

Products of carbonic acid and spirit obtained,

equal to 171.5 of alcohol.

The residuum 12 grammes, nauseous and, slightly acid.

Lavoisier says the acid is the acetous.

Residue of the ferment.

thus all the azot would disappear. What I had foreseen occurred: at the end of seven days, having filtered the liquor, I obtained as a residuum thirty grammes of a substance, which on distillation gave no trace of volatile alkali. I was persuaded, that the azot was carried off with the carbonic acid gas. To convince myself of this, I collected near 41 litres of the carbonic acid in an inverted vessel filled with solution of caustic potash. The whole was absorbed, which leaves no doubt of its purity.

Produced fermentation afresh, and thus lost all its ammonia.

This was not carried off with the carbonic acid.

What then becomes of the azot? it ought to be found either in the residuum of the ferment, or in the residuum obtained by evaporating the liquor left after distillation, or in the alcohol: but the residuum constitutes only half the ferment employed; the quantity of matter left by 300 grammes of sugar and sixty of ferment amounts only to twelve grammes; and neither of these yields any ammonia on distillation, while ferment affords a great deal. If these observations be just, if I have accurately noticed all the phenomena, if nothing have misled me, we cannot avoid concluding, that the azot must exist in the alcohol. Yet I have sought in vain to discover its presence in this fluid, in ether, and in the acetous acid: on passing these through tubes heated red-hot in the fire, and burning the gasses in Volta's eudiometer by means of the electric spark, we obtain such small quantities, that they are by no means sufficient to decide the question: 24 or 25 parts of gas yield at most one of residuum.

What became of the azot?

It must exist in the alcohol, though not discoverable there.

I have made several other experiments however, which hitherto tend to show, that azot may exist in such a manner as not to be discovered on distillation; consequently that it may be one of the constituent principles of vegetables, though in general when distilled they afford no ammonia. But as I have not repeated these experiments, and they are of such importance, that we cannot be too reserved in announcing them, I purpose to revise and vary them, I will endeavour to appreciate all the circumstances, and if I obtain convincing proofs, I will not delay communicating them to the class.

Azot may exist so as not to be detected by distillation, and therefore may be a constituent principle of vegetables.

However this may turn out, these results afford us sufficient light, to see what passes in the act of fermentation. In this respect I cannot agree in opinion with Lavoisier. I do not believe as he does, that all the carbonic acid formed proceeds from the sugar. How, on such a supposition, can we conceive

All the carbonic acid formed does not proceed from the sugar.

Ferment produces fermentation by taking from the sugar a portion of its oxygen.

Ferment has a strong attraction for oxygen, as is proved by its decomposing air.

What becomes of the principles of ferment.

Leaves a peculiar substance.

Yeast is seldom pure, containing more or less of this peculiar substance, with which it forms the chief part of wine-les.

the ferment to act upon it? I think, that the first portions of acid are owing to a combination of the carbon, of the ferment, and the oxygen of the sugar, and that the ferment gives rise to fermentation by abstracting from the sugar a portion of this principle. To render this idea more clear, I suppose a particle of sugar to be formed of eight parts of oxygen, four of carbon, and one of hydrogen, which is not very remote from the truth, according to the experiments of Lavoisier: one of these eight parts of oxygen will unite with a fourth of a part of the carbon of ferment, and then, the equilibrium between the principles of the sugar being disturbed, they will combine in a different manner, so as to form carbonic acid and alcohol.

The ferment has in fact a strong attraction for oxygen; as is proved by its decomposing air with the greatest facility; when acetous and carbonic acids are produced, and the azot is disengaged. If pure air be employed instead of common air, the reaction is still more speedy. I have introduced 15 grammes of ferment into a vessel filled with a litre of pure air; I opened it over quicksilver; a fifth of its bulk was absorbed; the ferment was grown sour, all the oxygen gas had evidently disappeared, and was converted into carbonic acid: the temperature was 15°.

From what has been said we see what becomes of the carbon of the ferment, and we shall learn what may become of its other principles, if we recollect the quantity of products resulting from a given quantity of matter subjected to fermentation, and comparing the nature of the one with the other.

From sixty grammes of ferment, and three hundred grammes of sugar we obtained, carbonic acid 95 grammes; pure alcohol 171.5; extractive matter, slightly acid, and containing no azot, 12; residuum of the ferment 40. These 40 grammes still contained 25 of ferment, so that 35 only had been employed for the decomposition of the sugar; and these 35

were reduced to 15 of a white substance* insoluble in water, incapable

* The yeast deposited by a fermenting juice is seldom pure: in general it contains more or less of this peculiar white substance, which is sometimes so abundant, that the sediment consists of it almost entirely. It is the mixture of these two substances, that constitutes in great part the lees of different wines. All these effects depend on the relative proportions of the ferment and the sugar; if there be little sugar and much ferment, the yeast will be pure; if

on

incapable of acting upon sugar, yielding no ammonia on distillation, and leaving a coal, that burns with scarcely any residuum; in short, exhibiting characters that distinguish it from all other substances, and induce me to consider it as a peculiar matter.

It appears then, that ferment takes oxygen from sugar, not only by means of part of its carbon, but also by means of part of its hydrogen; for the quantity of carbon given out by ferment is too small, to be the sole germe of fermentation. The azot disappears, and enters perhaps into the composition of the alcohol; the other principles of the ferment form acetous acid, and a peculiar insoluble white matter, which is precipitated. The acetous acid remains in solution in the liquor left after distillation, with an extractive matter, proceeding no doubt from the sugar, and foreign to it.

The hydrogen of the ferment acts as well as its oxygen in producing fermentation.

It is not probable, that the elements of the sugar, in their reaction upon each other when the equilibrium between them is disturbed, form water: there is very little hydrogen in sugar, and a great deal in alcohol; and besides, on adding together the quantity of carbonic acid, of alcohol, of extractive matter, and of residuum, we have a deficiency of one eleventh only of the matter by which they were produced. This loss must be attributed to the water the sugar contains, and is by no means owing to alcohol's being carried off by the carbonic acid. Of this I have been convinced myself by receiving more than thirty litres of this gas in caustic potash; and by distillation and rectification I obtained only a few grammes of fluid, which had so little taste of spirit, that it was not to be distinguished.*

The elements of the sugar do not form water.

A loss of one eleventh owing to water contained in the sugar. No alcohol carried off by the carbonic acid.

This

on the contrary there be much sugar and little ferment, the sediment formed will contain but little yeast, and perhaps even none; and then it will consist entirely of the white substance here mentioned, while the supernatant wine will or may be of a saccharine quality. Thus should yeast even be a product of fermentation, it would act an important part in this phenomenon; and if it do not give rise to it in the first place, which is not certain even in this case, at least it continues to give rise to it. This was the reason why I said before, that in all cases nature afforded us numerous applications of the theory I laid down.

This depends on the proportion of the ferment to the sugar.

* Cit. Seguin has laid before the Institute a very different theory of fermentation. He thinks, that in this process water is decomposed, Seguin's theory of fermentation.

This theory agrees with the facts, though not yet complete.

What remains to be done.

This theory, appears to me so much the more probable, because it perfectly accords with facts; and this truth becomes particularly striking when we compare them. I am far, however, from considering it as complete. Time no doubt will bestow on it the perfection it wants; and I hope myself soon to add to its evidence. I know not whether I shall be able to discover what becomes of the azot of the ferment: but I shall ascertain without difficulty, whether the residual matter obtained, which I consider as a peculiar substance, be a product of fermentation, as I believe; whether the sugar contribute to its formation, which is possible; or whether it be ready formed, and merely precipitated, which is contrary to all probability. The experiments, that remove all my doubts on this head, I shall relate in a second memoir, in which I shall not only elucidate such points in this as may appear equivocal, or at best resting on too slight foundations, but I shall also exhibit all the particulars that result from them. Here on the contrary I have avoided them as much as possible, and endeavoured to consider the phenomenon only in a general way.

VII.

Conjectures respecting the Formation of the Ice in the Cavern of Grace-Dieu. By C. L. CADET, of the College of Pharmacy.*

A cavern 146 feet underground, its entrance 60 feet broad and 80 high, its breadth 135 feet.

ABOUT six or seven leagues from Besançon, near the village of Beaume, and half a league from the abbey of Grace-Dieu, there is a natural grotto, 146 feet below the surface of the soil. It is supposed, that its oxygen unites with the carbon of the ferment, and produces carbonic acid, while its hydrogen combines with the sugar, and converts it into alcohol. For this theory to be admissible, we should obtain more alcohol than there was sugar; but the fact is, little more than half its weight is produced, and besides, supposing all the carbon of the decomposed ferment to be converted into carbonic acid, at most not above a sixth part of the quantity actually produced would be formed, as we may easily convince ourselves from calculations already established. It may likewise be objected to this theory, that sugar contains a great deal of oxygen, and alcohol very little.

* *Annales de Chimie*, No. 134, Feb. 1803, p. 160.

the plain. The entrance of it is 60 feet wide, and about 80 high. Within its greatest breadth is 135 feet. The stone that forms the rock is calcareous carbonate, in part lamellated.— This grotto is distinguished from all others by a very singular phenomenon: in summer ice is constantly formed in it in large quantities, and this ice diminishes at the approach of winter.

Ice is formed
it in summer, &
and diminishes
in winter.

As this natural ice-house has been accurately described by Mr. de Croismare in the *Encyclopédie*, and several other descriptions agreeing with his are to be found in the *Memoirs of the Academy of Sciences* for 1712, those of the *Savans Etrangers* for 1743, the works of Mr. le Cat and Ravier, and Madame Ganthier's *Tour of a French Lady in Switzerland*, I shall confine myself to a few observations, that have escaped the notice of many of the learned who have visited it, and combine to explain the formation of the ice in this cavern.

Described by
various writers.

I visited it about the end of September, 1791, and then found but little ice. My guide, who was accustomed to conduct travellers to it, informed me, that a month before the ice was three times the bulk of what I saw. The person who sells refreshments at Chammar, a public garden at Besançon, told me the same. He said, that when the winter had proved mild, so that they were not able to fill the ice-houses in the city, he sent in search of ice to the cavern of Grace-Dieu, and that he chose for this the warmest days, *because then there was most ice in the cavern*. Messrs. de Croismare, le Cat, and Ravier, in their accounts, equally attest, that in summer the ice is more abundant. The inhabitants whom I met with on the mountain, and of whom I enquired concerning this fact, all told the same thing. Consequently, though the variation of the ice is questioned by Mr. Dunod, quoted in the work of Madame Ganthier, I readily admit the existence of this phenomenon, particularly as I think I can explain it.

In September,
1791, it had but
little ice.

A month be-
fore it had thrice
the quantity.
Ice there in mild
winters.

By several au-
thorities the ice
is most plentiful
in summer.

The rock that forms the roof of this grotto is lower than all the neighbouring mountains, and even than the surface of the plains. Its temperature therefore ought to be mild, like that of all subterraneous caverns, except at the moment when ice is accumulated there in abundance. Ten waggons would have been sufficient to carry away all I saw, and the air of the grotto did not feel colder to me than the open air. The authors I have quoted say, that the cold in it is below 0. I had no thermometer, but I can affirm that this assertion is at least questionable.

The rock that
forms the roof
low.

The air in it
does not feel
cold.

Description of
the cavern.

Clear water
bursts through
the rocks and
falls into a basin
of ice.

The grotto is
covered by
bushy trees.

The ground is
cooled by the
evaporation in
summer,

which does not
take place in
winter.

The trees over
the entrance
being cut down,
less ice was
formed.

The entrance
was partly closed
up by a wall,

by which the
ice was dimi-
nished.

A just idea of the situation of the cavern is sufficient to shew in what manner the water percolates the strata of rock, and falls perpetually from the roof. The cavern is surrounded by thickly wooded mountains, and from one of the nearest rocks rushes a torrent, that forms three cascades, and turns several mills. But what is remarkable is, that the water filtering into the cavern does not freeze as it falls: it is received into a basin of two or three feet diameter formed in the ice, and remains fluid on a level with the sides of the basin. I drank some of this water, which did not seem to me very cold. It was perfectly clear and good.

I observed, that the grotto is not only surrounded by wood on all sides, but that it is covered by a number of the most beautiful beech, and bushy chestnut trees. These deny all access to the rays of the sun, and always preserve a cool damp air round the cavern. This shade however would not be sufficient to produce an icy air, but the leaves of these trees are so many organs of perspiration, that pour into the atmosphere the moisture which exhales from their vessels, and that has been pumped up by their roots. This evaporating water cannot pass from the state of a fluid to that of a gas, without absorbing a large quantity of caloric: the ground therefore is continually cooled by the evaporation the trees produce. In winter the warmth of the sun no longer draws up moisture; the leaves, which botanists consider as real lungs, have discontinued their organic action; and the same evaporation no longer exists. The temperature of the cavern then changes, and approaches that of subterranean places and grottos in general. Several facts serve to confirm this theory, and to prove, that the cold produced in the cavern of Grace-Dieu is entirely owing to the speedy evaporation of the surrounding moisture.

Mr. Ravier, secretary to the bishop of Bellay, says, that in 1724, the entrance of the grotto was overshadowed by large bushy trees, that these were cut down, and that the quantity of ice afterwards formed has been less.

He also reports, that Mr. de Vanolles, intendant of the province, desirous of preserving this natural curiosity, closed up the grotto by a wall twenty feet high, with a small door, the key of which was entrusted to the bailiffs of the village, with a charge, that, no one should be suffered to enter for the purpose of carrying away the ice. But this precaution produced

an unexpected effect: the ice perceptibly diminished, and they were obliged to pull down the wall, which was in some measure an obstacle to the evaporation. I saw the remains of this wall, and the inhabitants of Grace Dieu confirmed to me the fact mentioned by Mr. Ravier.

The formation of the ice in this curious grotto, therefore, may be explained, I believe, according to the theory of the jars that serve to cool the water they contain in hot countries, by permitting part of the fluid to transude through their porous sides.

This ice stated to be formed in the same way as water in porous jars is cooled in hot countries by the evaporation from the outside.

As no natural philosopher has yet attempted to explain this interesting phenomenon, I have flattered myself that I might be indulged in a conjecture, founded on physical principles fully established by experience.

VIII.

Observations on the Structure of the Tongue: illustrated by Cases in which a Portion of that Organ has been removed by Ligation. By EVERARD HOME, Esq. F. R. S.*

(Concluded from Vol. VI. Page 379.)

ENCOURAGED by the result of this case, I was led to perform a similar operation upon a person at a more advanced period of life.

Another case treated in the same manner,

Margaret Dalton, 40 years of age, was admitted into St. George's hospital, on the 25th of December, 1801, on account of a tumour, the size of a pea, situated on the right side of the tongue, near its edge. The history of the case was as follows. A small pimple appeared, and gradually increased, without pain; the only inconvenience was, that it affected her speech, and, when bruised by the teeth, bled freely.

The operation was performed on the 11th of January, 1802, in exactly the same manner as has been already described. It produced a considerable degree of salivation, which was extremely troublesome, (much more so than the pain the ligatures produced,) and continued till the slough came away. The ligature nearest the root of the tongue separated on the 6th day; the other on the 7th; and, in three days after the separation of the second ligature, the wound was completely skinned over.

with complete success.

A third case.

A third case of this kind came under my observation, in which there was a small tumour in the substance of the tongue, about the size of a pea, which gave me the idea of its being of that kind which might terminate in cancer. The patient was a gentleman of about 41 years of age. Upon examining the tumour, I told him of my alarm respecting its nature; and at the same time added, that I was very ready to remove it, should it be the opinion of other practitioners that such a step was adviseable; and my experience in two former cases led me to believe it might be done with safety. I therefore advised him to consult other medical practitioners of reputation, and acquaint me with their opinion. Mr. Cline was consulted, and his opinion coincided with mine; which made the patient decide upon having the tumour removed.

The operation,
and history of
its progress and
successful termination.

The operation was performed on the 28th of December, 1802. The needle pierced the tongue an inch beyond the tip, a little to the right of the middle line of the tongue; and the space between the two ligatures, when they were tied at the circumference of the tongue, was fully an inch. The tongue was thick; and the mass included by the ligatures was such as to make it difficult to compress it. The operation gave considerable pain, of a numbing kind. Immediately after the operation, the part included became dark coloured, particularly towards the middle line of the tongue. A salivation took place. The next day, the pain and salivation were great, and the patient could not swallow; but, on the day following, he could take broth, negus, and other fluids.

On the 6th day from the operation, the slough became loose; and the least motion of the tongue gave great pain. Upon examining the slough, there was a small spot which looked red, and was surrounded by a dark surface; this was towards the right side. Upon further examination it appeared, that the ligature to the right had not completely deadened the part at the centre, in which the artery had its course. This accounted for the red spot, as well as for the pain the patient suffered; and led me, on the seventh day, to disengage the ligature on the left, (which was almost completely separated,) by means of a pair of scissors, and pass another ligature through the groove to the opposite side, and tie it over the part not completely deadened. This gave great pain for a few hours, which was relieved by the use of tincture of opium.

opium. On the 8th day, the patient had less pain than on any preceding day, and less salivation; and, on the 9th, the whole slough came away. On the 13th, the tongue had so much recovered itself, that there did not appear any loss of substance whatever, only a fissure of half an inch in depth, in the anterior part of it; and, as that now seemed to be exactly in the centre, there was not the smallest deformity.

The preceding cases, in the view which it is intended to take in the present Paper, are to be considered as so many experiments, by which the structure of the tongue is in some respects ascertained: they enable us to draw the following conclusions.

General results of the preceding cases or experiments.

The internal structure of the tongue is less irritable than almost any other organized part of the body; therefore, the peculiar substance which is interposed between the fasciculi of its muscular fibres, is not in any respect connected with the nerves which pass through its substance to the organ of taste, but is merely a soft medium, to admit of great facility of action in its different parts.

Internal structure of the tongue has little irritability.

The nerves of the tongue appear to be more readily compressed, and deprived of their power of communicating sensation, than nerves in general; and any injury done to them is not productive of diseased action in the trunk of the injured nerve.

its nerves easily deadened.

If we compare the effects of compression upon a portion of the tongue, with those of a similar compression upon the hæmorrhoidal veins when they form piles, or those of the testicle in cases of varicose veins of the spermatic chord, which not only produce very violent local inflammation, but also a considerable degree of symptomatic fever, it is impossible not to be surprised that the results should be so very different; since we are led to believe, upon a general principle, that parts are sensible in proportion to their vascularity, and that all the organs of sense, when inflamed, are more exquisitely so than any other parts of the body.

Comparatively little inflammation and pain follow compression of this organ.

The tongue appears to have a power of throwing off its sloughs in a shorter time than any other part. Eight or nine days is the ordinary time of a slough separating from the common parts: in the boy's tongue, it was only five.

It sloughs off very early.

Having stated the information we derive from these cases, respecting the structure, sensibility, and irritability of the

tongue, it now remains to mention the advantage to be derived from them in a professional view; and, although this is not directly in the line of the pursuits of this learned Society, yet, so strongly is it connected with humanity, that it cannot be said to be foreign to them, or undeserving their attention.

Great benefit of this knowledge in cases of cancer.

The information derived from these cases, enables us to attempt with safety, the removal of any part of the tongue which may have taken on a disposition to become cancerous. As this disease in the tongue always begins in a very small portion of that organ, it is, in the early stage, more within the reach of removal than when in any other part of the body; and, as the glands of the tongue are independent of each other, the cancerous disposition by which one of them is attacked, does not so readily communicate itself to the others; and the part may be removed, with a greater degree of security against a future recurrence of the disease, than in other cases where this malady attacks a portion of a large gland, the whole of which may be under the influence of the poison, long before there is any appearance of its being diseased.

IX.

Account of Three Mechanical Structures not commonly noticed or known, in which the Effect is produced by the Difference between the Actions of Two simple Instruments of the same Description; namely, 1. a compound Barrel and Winch; 2. a compound Screw, and 3. a compound Pump.

W. N.

Properties of the wheel and axle.

IN the simple mechanical engine, called the wheel and axle, or barrel and winch, the advantage of the power over the weight is known to be as the radius of the wheel or winch is to that of the axle or barrel. If it were, therefore, proposed to increase that advantage, the handle must either be made longer or the barrel more slender; neither of which can practically be done beyond certain limits in the usual form; because the handle might be too long for management, or the barrel too slight to support its burthen. The celebrated George Eckhardt is, I believe, the inventor of the construction, *Plate III. Fig. 2.* in which the part A of the barrel is larger than the part

A compound barrel or roller, by which the purchase may be greatly augmented.

part B, and the rope which passes under the pulley C and sustains the weight D is wound upon each in contrary directions. Whenever, therefore, the handle is turned, so as to gather the rope upon the larger cylinder, it will be given off by the smaller: and, for every turn of the larger or its correspondent portion of rope wound up, there will be given off a portion of rope, answering to the circumference of the smaller. Consequently, the quantity of unwound rope will be less, after such a turn, by a quantity equal to the difference between the circumferences of the two cylinders; and the weight D will be elevated through half that space. Whence

As the circumference described by the winch (or its radius) *Its power*
Is to half the difference between the circumferences of the
cylinders (or half the difference of their radii)

So is the power

To the weight (in equilibrio.)

It is evident that the peculiarity of this engine is that the half *Peculiarity of this engine.*
difference of the radii may be made as small as we please without weakening the cylinder or requiring any quick curvature of the rope; neither of which requisites could be had in the simple cylinder. Its practical disadvantage appears to be that a large quantity of rope will be used to produce a moderate elevation of the weight.

The first account I find of the double screw *Plate IV. Double screw engine.*
Fig. 1. is given by Mr. Wm. Hunter in the Philosophical Transactions for 1781. In the sketch before us, A B C represents *One screw presses down while another draws up, and the effect is produced by the difference of their actions.*
the screw of a press: the upper part of the screw A has a coarser thread than the lower B and a concave screw or nut, C, receives the latter, while the former passes through a proper helical cavity in the frame of the press. In the present state of the apparatus, the nut C is pinned fast to its screw by a cross bolt; and consequently, when the whole screw is made to turn by the arms at the top, the nut C is carried round, and the effect is precisely the same as if the press were governed by the screw A alone. But when the nut C is disengaged by drawing the cross bolt, and likewise prevented from revolving by pinning it to the moveable platform, the effect becomes more compounded: that is to say, while the whole screw is carried downwards by one revolution, through one thread of A, the nut C is drawn upwards through one thread of C. And

Examples of
slow actions
with strong
coarse screws.

the consequence is the same as if a single screw had been used, having the distance between thread and thread equal to the difference between the measures of one thread in each.

For example. Suppose it were required to operate by strong pressure with a screw of ten threads in the inch, it is evident that the metallic protuberance or helix could not be quite so thick as one twentieth of an inch, and could not therefore withstand any considerable force. But by the present construction we may make the thread as strong as we please. Thus if the slightest be one quarter of an inch, the finest screw must have two turns in the inch, and the coarsest two turns in one inch and one tenth; or in other words, it would pass over the same space in ten turns as the other screw would in eleven.

It is evident that by the construction in the figure, the most powerful pressure need not be used till the subject under compression has already been urged by the simple screw.

Observation on
new or unusual
constructions.

I may here remark concerning every one of the contrivances in this paper, that I have never considered it as a requisite in offering a machine or instrument to the attention of my readers that it should be universally better than the means already in use. If it be new or unheeded, and ingenious, or capable of adding to the stock of resources which intelligent constructors find so various in their degrees of eligibility under different circumstances, it will, I am confident, be considered with interest, and accepted with welcome.

Considerations
respecting the
use of a double
screw in mea-
suring.

When we consider the extreme precision of a simple screw, if commonly well made, it may seem scarcely necessary to use this contrivance in the measurement of small quantities. The finest screws usually to be met with do not exceed 100 threads in the inch, and I do not know of any having been made finer than of 200 threads in that space. Let us consider by what threads of usual or even coarse number we could have a difference per turn of above one thousandth of an inch, and we shall find that the difference between a thread of 32 and one of 33 in the inch is the 1056 part of an inch. So likewise the difference between the 60th and 61st of an inch is the 3660th part, &c. I have a level trier on this construction which gives the four hundredth of an inch for each turn of its compound screw.

Pump with a
double plunger
by which the

Fig. II. Plate 4. is a pump described to me by Mr. Bramah, and I think his own invention. Between the collar of leather,

A, there passes a solid cylinder, of which the lower part B (diminished in diameter) passes out through another collar of leathers D; the internal space or pump barrel situate between those two collars being closed on all sides, except where two pipes communicate, one of which, E, has a valve opening inwards, and the other D, has a valve opening outwards. Now when the solid cylinder or plunger is drawn up, the larger portion passes for the most part out of the cavity of the barrel; but instead of being replaced by an equal mass of water entering through the pipe E as usual in the jack head pump, it is mostly replaced by the introduction of the solid part B, and the water which enters will be equal in its solid contents to the difference only between the solidities of the two cylindrical parts of the same length. Or the effect will be the same as if the barrel were of much smaller dimensions. In this way with the strength of construction and ample fittings of a large engine the power of a small one may be obtained, which in some instances of forcing may be desirable.

It scarcely need be mentioned that the same effect may be produced by two solid pistons working in distinct barrels of different bore or stroke, and communicating with each other.

X.

Scotography, or the Art of Writing in the Dark. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

PERHAPS you have remarked that my communications to your Journal are not all in the same hand: the truth is, I am unable to write; and my ignorance of the art arises from the loss of sight in early infancy. The knowledge of the preceding circumstance cannot prove of the least moment to you; it is only mentioned by way of introduction to the present letter, which may afford an useful hint to persons labouring under the same disadvantage.

The perusal of Mr. Berard's method* of teaching the blind

* Phil. Jour. Nov. 1802, Vol. III. page 190.

to write was perhaps the first occurrence of my life which led me to think seriously of committing my thoughts to paper with my own hand.

Berard's method of writing by the touch only is not adapted to the early blind.

This gentleman's plan however presented many difficulties which it is unnecessary to enumerate; in reality it promises to be of much advantage to those who have learned to manage a pen before they lost the use of their eyes: but as this has not been my lot, I resolved to conquer the hardships of fortune, if possible, by inventing an alphabet consisting of characters which a mechanical apparatus might form with ease and certainty. After sketching an outline of a method in my own mind, I imparted my unfinished project to an ingenious friend, being induced to ask his assistance by a consciousness of my own inexperience in the art of writing. This gentleman completed the business in a short time, by inventing a set of distinct characters of easy execution, and contriving the means of imprinting them upon paper.

Requisites of the art.

The origin of the invention being now stated, the process may be described in the following manner; including the preparation of the paper, the alphabet, the law of combining the letters, the apparatus, and the proper kind of ink.

Practice.

The paper is ruled into square compartments.

The paper is prepared by dividing it into several square compartments, which will appear in the annexed figure, page 57. The operation might be performed by means of a ruler, but the page may be stamped with greater certainty by the writing-frame; which instrument will be described in the sequel.

The alphabet is formed by placing a dot or a stroke in a different part of the square for each letter.

The alphabet is formed by means of two marks only; these are a dot, and a straight stroke; for each compartment may be considered as the skeleton of any character the writer wishes to make; and it is completed by placing the requisite mark in its proper place. The letters might be described separately, but a moment's inspection of the alphabet given in *Fig. 1st* explains the structure of it better than words can do. The scheme moreover shews, that there are thirteen places where the marks may be fixed, so as to form all the letters: to these may be added the three characters denoting the end of a word, a capital, and the numerals 1, 2, 3, &c.

Method of combining the letters or of writing.

Since placing a dot or a stroke in a square makes a letter, the same compartment may form the basis of several characters in certain circumstances; provided the law of combining them be first established.

My

My method consists in supposing the thirteen places of each compartment to be numbered, as is done in the annexed figure (Fig. 2d.) Now if, in spelling a word, the place of the former of two adjacent letters be of a less denomination than that of the latter, they may both be put in the same square; in like manner if the second do not exceed the third in value, this last may also be inserted with the other two; and the same rule will obtain with the fourth, &c. Letters of the same denomination, as o, r, &c. are to be placed in separate squares; and every capital must have a compartment to itself. This way of writing observes no proportion betwixt the length of a word and the space occupied by it: the monosyllable (and) fills three squares; which is the number taken up by the trisyllable (inspection) See Fig. 1st.

The elementary processes of the art being now explained, we come in the next place to the description of the instruments and their uses.

The writing-table is an oblong board, having two parallel brass plates passing from bottom to top, and containing a space between them, just sufficient to receive a sheet of paper: they are an inch in breadth, and $\frac{1}{8}$ of an inch in thickness, being fixed with their flat sides to the table. A row of short pins is placed along the middle of either plate: each pin in one row is opposite to one in the other, and the distance of two adjacent pins in either plate, is exactly equal to the breadth of the writing frame. This instrument is an oblong piece of brass, a quarter of an inch thick: it has a bevil at each end, which parts pass over the side plates of the table, and thereby suffer the intermediate parts of the frame to rest upon the paper. The annexed figure (1st) shews the structure of the perforated part and the size of the squares.

The process of stamping the paper consists in covering the underside of this plate with printer's ink; a small hole being made in either bevil for the reception of two opposite pins in the side plates; the operation commences by placing the frame on the highest pair, it is afterwards successively removed to each inferior pair until the whole page is stamped and prepared for the pen.

This instrument is a square brass rod, four inches long: each side of the square being nearly half as long, as a side of

of a compartment in the frame. One end of this pen is cut away parallel to one of its sides, so as to form a plate three eighths of an inch long and a line in thickness: the end of the plate is also filed down so as to leave at one corner a point, which is parallel to the axis of the rod: this point is used in making dots. The other end of the rod is cut diagonally parallel to its axis, thereby forming a prism, the end of which is an isosceles triangle. The extremity of this prism is also cut away, so as to leave a plate lying in the same plane with that described above: the end of this plane has a small bevel externally, which gives it the edge of a chisel; and it is used in making strokes. The writing frame always rests on that part of the page, which you are going to write upon; and the sides of the compartments together with their angles guide the pen to the proper places: the method of using which will easily occur to those who are acquainted with the alphabet and apparatus.

Ink.

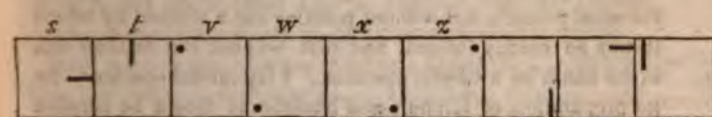
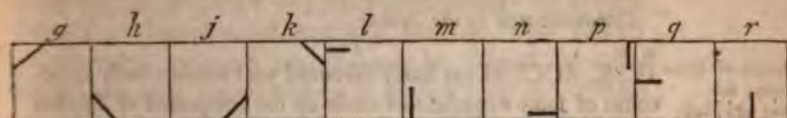
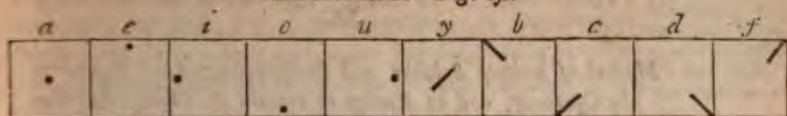
They should however be informed that *printer's ink*, is fittest for the purpose: it is spread upon a cushion of soft leather, which must be touched by the pen, as oft as it is applied to the paper.

JOHN GOUGH,

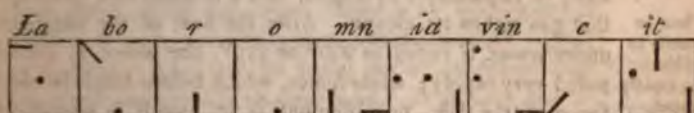
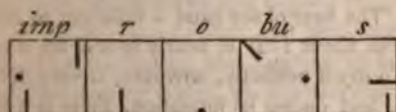
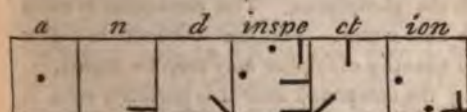
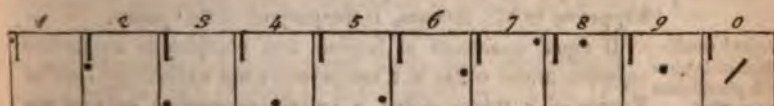
*Middleshaw, Dec. 15, 1803.**(See alphabet, next page.)*

P. S. A paper of mine was read Nov. 4th before the Manchester Society, in which I endeavour to demonstrate Mr. Dalton's doctrine of mixed gases to be repugnant to the mechanical philosophy. It also proves experimentally that water augments the bulk of air of a given temperature without the intervention of a stopple of mercury; from which the non-existence of an aqueous atmosphere is inferred upon known principles.

ALPHABET.

ALPHABET. *Fig. 1st.*

* ** ***

*Fig. 2.*

1	12	110
2		
3	13	9
45	6	8
		7

Denominations of the Letters.

1	2	3	4	5	6	7	8	9	10	11	12	13
b	l	i	c	m	o	d	n	u	f	p	e	a
g	q	h		r	j	s	k				y	
v		w			x							z

* The square distinguished by one star underneath contains the mark to denote the end of a word.

** The square so distinguished contains the mark denoting a capital letter.

*** The square so distinguished contains the mark signifying a numeral character or number.

XI. Method

XI.

Method of uniting Sulphur and Phosphorus without Danger to the Operator, and an Attempt to explain the Change that then takes Place. By ROBERT BRIGGS, M. D. Edinburgh. Communicated by the Author.

Danger of combining sulphur and phosphorus.

MR. ACCUM has lately favoured your readers with an account of some experiments made on the compound of sulphur and phosphorus, showing the great danger of preparing it by the usual process, but without pointing out a method by which it may be readily formed, and that without the smallest risk in the hands of a careful operator. I say careful operator, for no preparation of sulphur and phosphorus should be handled carelessly. In order to prevent effects similar to those which happened to Mr. Accum, is the purport of this paper.

Limited combination of sulphur with phosphorus under 180°.

If equal quantities of sulphur and phosphorus are melted together under water at a temperature not exceeding 180° of Fahrenheit's thermometer, a certain proportion only of the sulphur unites with the phosphorus. The remainder is easily washed away with cold water. This does not hold in higher temperatures: any quantity of sulphur may then be united.

Qualities of this compound.

Upon examining the compound after it is perfectly cold, it will be found to have a friable texture, spongy appearance, and a sulphur colour. The heat of the hand is sufficient to melt it, and a temperature of about 112° or nearly so will set it on fire if perfectly dry. To dry it perfectly, however, is very difficult. When just formed, and during its formation, there is a strong smell of sulphurated hydrogen, and a considerable quantity of that gas makes its escape. After the heat of the compound under water is raised to 200° or 210°, the water is decomposed very rapidly; white fumes, which before filled the empty space of the flask, now issue out of its mouth in abundance, and if the vessel is shaken by the hand or by allowing the water to boil, a sudden inflammation and explosion is the consequence. The compound of sulphur and phosphorus begins to decompose the water immediately after the union of the two commences, but, towards the boiling point of water, that decomposition is very rapid.

If the heat be raised nearly to boiling, the water is rapidly decomposed, white fumes escape, and it explodes.

If

If you take the vessel of the fire just when the white fumes have filled the flask, but, before they begin to issue from its mouth, and place it in a dark room without shaking it in the least, the whole space above the water which is filled with the dense white fumes, has a beautiful appearance, like an aurora borealis. At this time very frequently, though not always, bubbles of phosphorated hydrogen gas, or rather sulphur phosphorated hydrogen gas, if I may be allowed the term, escape and take fire on coming in contact with the external air, forming a beautiful white ring of smoke rising to the ceiling of the room,—just such as happens from the decomposition of water by means of phosphuret of lime or potash.

The white fumes are beautifully luminous in the dark, and fiery rings sometimes appear.

Now what is the change produced during this formation? That the water is decomposed, is abundantly evident. But while the hydrogen of the water escapes, what becomes of its oxygen? If the oxygen were also allowed to escape, we should feel its presence, from explosion after explosion taking place, immediately after it left the water along with the hydrogen, united with as much phosphorus as is sufficient to fire the two.

In this process, though the water is decomposed, the oxygen is not set free.

The firing, however, only happens after the sulphur phosphorated hydrogen reaches the external air, or when the decomposition goes on very rapidly from a high temperature, and faster than the oxygen can be disposed of. From all these facts taken together, I believe it must be pretty obvious, that the compound which we form of sulphur and phosphorus under water, is not of pure sulphur and phosphorus, but of their oxides. That is, during the junction of sulphur and phosphorus by heat under water, part of the water is decomposed, the hydrogen escapes after engrossing a small portion of sulphur and phosphorus, while the oxygen unites with the sulphur or phosphorus, or both, converting them into an oxide of sulphur and phosphorus. This will be found farther confirmed from considering the nature of this compound. It is very inflammable, much more so than a compound of pure sulphur and phosphorus formed by another method which I shall describe by and by; but which may be converted into an oxide by a very simple process, which I shall also point out; then it becomes one of the most inflammable bodies with which I am acquainted. Farther the compound of oxide of sulphur and phosphorus, does not decompose cold water so far as I have been able to perceive, though the contrary is said by Mr. Accum; but it will decompose the water if heated.

It is employed in oxidizing the bases.

which oxide is more inflammable than the simple compound.

Phosphorus and sulphur do not decompose water unless heated.

Upon

The compound first mentioned is of the oxide; and the phosphorated and sulphurated hydrogens contain oxygen.

Best method of forming it.

The mixture heated in a phial covered with a perforated bladder.

Explanation of Mr. Accum's accident.

Upon the whole then, I imagine, that, this compound when formed under water, is nothing else than a mixture of the oxide of sulphur and the oxide of phosphorus, and that phosphorated hydrogen gas, is not phosphorus dissolved in hydrogen gas, but a solution of the oxide of phosphorus in that fluid, and the same may be said of sulphurated hydrogen. It is not sulphur, but an oxide of sulphur that is dissolved.

I shall now proceed to mention what I consider as by far the best method of forming a junction of sulphur and phosphorus, and of afterwards converting them into oxides without risk or danger.

When I first began to examine the nature of the oxides of sulphur and phosphorus, the decomposition of water, and the escape of its hydrogen only, at a low temperature, I imagined that I could effect the union of the two much better without the water.

I took a phial glass and filled one third of it with the usual proportions of sulphur and phosphorus, I tied a piece of strong bladder over the mouth of the glass, and made a small hole in it with a pin. I expected that after the phosphorus began to melt, probably it would fire with the small quantity of oxygen of the atmospheric air in the phial, before the sulphur and phosphorus had joined. I therefore used the precaution of a small hole in the bladder to allow the escape of the rarefied azotic gas. It did fire, and was followed by a trifling explosion.

The heat was by this suddenly augmented, the union of the sulphur and phosphorus formed immediately, and nothing more particular happened. The heat, however, is raised so suddenly that if the phial is large it is very apt to break. I would not therefore recommend this to be tried by a young chemist without great caution.

Before going farther, I would now attempt to explain the accident that happened to Mr. Gardenas related by Mr. Accum. The compound of oxide of sulphur and phosphorus which he was melting with the phosphorus, had either not been thoroughly freed from water, which it is very difficult to do, and hence the explosion is easily explained on the principles which I have already related; or, allowing that there was no water, the phosphorus had been fired by the oxygen of the atmospheric air in the upper part of the phial, at the very moment of shaking

Making the glass. This of course would raise a quantity of the melted phosphorus, and at the same time oblige him to remove his finger from the mouth of the phial. The highly rarified nitrogen would rush out with violence, carrying part of the melted phosphorus along with it, an explosion and its effects would be the consequence.

A pretty loud report may be produced by heating any portion of air in a phial glass covered by the finger, on suddenly removing the finger.

This explanation, however, is rendered abundantly evident from what follows. Fill a phial glass, or what is still better, a thin glass tube shut hermetically at one end with sulphur and phosphorus. Cork it firmly, and plunge the tube into hot water, the heat of which is to be gradually augmented till it boils. The sulphur and phosphorus will melt and unite peaceably. Shake them after taking the tube from the boiling water, that the mixture may be complete, and no accident will happen.

Experiment in proof. Phosphorus and sulphur gradually heated, in a small closed vessel, unite without danger.

It is hardly necessary to caution against plunging the tube into boiling water at first. A contrary plan allows any small quantity of oxygen that may be present in the very small portion of air that may remain between the particles of the sulphur and phosphorus to be taken up by the phosphorus before melting. If a compound of sulphur and phosphorus be prepared as above directed, having only a small proportion of sulphur, it has a yellowish white colour, is solid when cold, and has a crystallized appearance. This substance is more inflammable than phosphorus, but not by any means so much so as a dry oxide of sulphur and phosphorus. This compound, however, may be easily converted into an oxide by the following simple process. Set fire to the mixture, still in the tube, by taking out the cork, admitting a little air, and plunging a hot wire into it; allow it to burn only five or six seconds. It will by this be converted into the oxide of sulphur and phosphorus, be found to resemble in appearance very much a neutral salt efflorescing, sublime into the neck of the glass, and be so very inflammable that the instant you bring it into the air it catches fire, though the thermometer be at the freezing point.

Not so if the heat be sudden.

This compound is less inflammable than the first mentioned oxide:

into which it may be converted by combustion.

Edinburgh, December 15, 1803.

XII.

On the best Method of ascertaining the Dip at Sea. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON,

SIR,

The dip of the horizon rendered very uncertain by refraction.

The back observation will not correctly shew the dip.

It is best found by taking the sun's altitude and its opposite supplement by two persons ;

or by one person with two instruments.

IT is well known that refraction varies so much near the horizon as to render the depression or dip very uncertain, whence it seems desirable, in the present improved state of navigation, to find the dip by observation rather than trust to the table. An instrument so constructed as to measure an angle of 180° would be very convenient for this purpose, and the back observation in Hadley's quadrant has been proposed ; but the back observation is attended with so many inconveniences to the observer, and so little to be depended upon, as to render it nearly useless. The instrument which I have proposed for taking any angle less than 180° seems well adapted to this purpose *. The distance of the two opposite horizons at sea is $= 180^\circ +$ twice the dip ; but the motion of the ship renders this method very troublesome and uncertain ; for whatever uncertainty attends taking an altitude of a celestial object, that uncertainty is doubled in taking the distance of the two opposite horizons. To avoid this inconvenience, let the meridian altitude of the sun's upper or lower limb be taken with a sextant by one observer, and at the same time, let another observer, with an instrument so constructed as to measure an angle of 180° take the distance of the same limb of the sun, from the opposite horizon ; the sum of these two angles— 180° is $=$ twice the dip. And as two observations of the sun's altitude are thus taken independently of each other, the latitude will be obtained with greater precision than by a single one.

The observations may also be taken with two instruments by one person.

Let the observer begin some time before noon, and take the altitude of the sun's limb. 2. With the other instrument take the distance of the same limb from the opposite point of the

* See Philosophical Journal, vol. iv. p. 218. and vol. vi. p. 219:
horizon,

horizon, then take the first instrument, and correct the observation; and so continue to use the instruments alternately till the sun shall arrive at his greatest altitude.

If the sun's altitude were taken, when off the meridian, by two observers, in the manner above mentioned, in order to find the hour from noon, it might greatly add to the accuracy of that important problem. This method is recommended in finding the time.

REMARKS.

From the above strictures it appears, that navigation might receive many improvements from an instrument that would take in the largest angle, and admit of being accurately adjusted. The latitude and time of the day might then be determined with greater precision than at present, and the limits of the method of finding the longitude by the lunar theory would be enlarged.

EZEKIEL WALKER.

Lynn, December 22, 1803.

(To be continued in a future communication.)

ERRATUM.

Vol. VI. p. 219. near the bottom, for 120°, read 180°.

XIII.

New Process for fabricating Alum artificially and without the Assistance of Evaporation. By Cit. CURAUDAU, Corresponding Member of the Society of Apothecaries of Paris.*

A REPORT was made to the National Institute in Fructidor of the year 9, by Citizens Guyton and Vauquelin, on a new process of Citizen Curaudau, for the artificial fabrication of alum. As this report is not generally known, we hasten to communicate it to our readers.

Citizen Curaudau proposes, in the fabrication of alum, to employ one hundred parts of clay, and five of muriate of soda, diluted with a quantity of water sufficient to give a pasty consistence to the mixture. It is then made into loaves, with
Process employed by Cit. Curaudau to make alum artificially. 100 clay and 5 common salt kneaded with which water; the loaves

* From the *Annales de Chimie*, Floreal, An. XI: No. 137.

are ignited,
cooled, pounded,
and one fourth
of sulphuric acid
water is added
by degrees till 10
times the acid.

which a reverberatory furnace is filled, and wherein a brisk fire is kept up for two hours, or until the inside of the furnace is obscurely red. The calcination being finished, the clay is reduced to powder and put into a sound cask; one fourth of its weight of sulphuric acid is then poured over it, at several intervals, agitating it strongly upon each addition. As soon as the disengaged vapours of muriatic acid are dissipated, a quantity of water equal to that of the acid is added, and it is stirred as before. This produces a combination between the acid, the earth, and the water, so rapid, that the mixture heats, swells and emits abundance of vapours. Lastly, when the heat has abated a little, water is continually added until the quantity amounts to about eight or ten times that of the acid.

To the clear
decanted liquor
potash is added.

When that part of the earth which is useless in the formation of the alum is deposited, and the liquor become clear, it is drawn off into vessels or boilers of lead. A quantity of water equal to that of the liquor drawn off, is then poured over the residuum, and afterwards added to the first: lastly, a solution of potash is mixed with the lye, in which the alkali amounts to one fourth of the weight of the acid employed, and it is agitated. If the preference be given to employ sulphate of potash, the quantity used must be double that of the alkali.

The alum crys-
talizes by cool-
ing.

After a certain time, the liquor by cooling, affords crystals of alum, the quantity of which, when the crystallization is completed, amounts to three times the weight of the acid made use of. This alum is refined by dissolving it in the smallest possible quantity of boiling water; and it is then as pure as the best alum of commerce.

The washings
employed in a
subsequent
operation.

As the residue still retains some saline parts, Citizen Curau-dau recommends washing it a third time with a quantity of water sufficient to extract all the salt, and to use this instead of simple water in a second operation; by which means, there will not be any loss.

The mother-
waters useful
in making
prussian blue.

Thus, without the assistance of heat, the greatest part of the alum formed in the operation is obtained, which is highly beneficial. The author recommends the use of the mother-waters, which still contain alum with sulphate of iron very much oxidized, in the fabrication of prussian blue, to which purpose they are well adapted.

He

He considers the fabrication of alum as particularly advantageous to the manufacturers of prussian blue, because they may calcine their clay at the same time with their animal matters, without any increase of expence. They will have no need in that case to add potash; and the presence of iron, instead of being injurious, will, on the contrary, be very useful. If they are desirous of fabricating alum for sale, they may use the solution of sulphate of potash arising from the washing of their prussian blue, and which is usually lost, instead of water, to dissolve the combination of alumine and sulphuric acid. The residuums or refuse of the distillers of aquafortis are equally applicable to the same purposes: they contain the alumine and potash requisite to the composition of alum, and it will be sufficient to sprinkle this substance, reduced to powder, with sulphuric acid, and to add the necessary quantity of water to the mixture, proceeding as has been directed above. The mother-waters of these alums are also useful in the fabrication of prussian blue.

This process very beneficial to the makers of prussian blue.

Refuse of the distillation of aquafortis may be used,

We may also observe on this occasion that the refuse of the distillers of aquafortis contains more alkali than is required for the saturation of the sulphate of alumine formed by the clay, and that to obtain the greatest possible quantity of this substance, it is necessary to add an eighth by weight of this earth calcined, according to the process of Citizen Curaudau; and that in using about sixty parts of acid to a hundred of this earth, at least one hundred and eighty parts of very fine alum are obtained.

It contains an excess of alkali.

Citizen Curaudau affirms, that he has for a long time prepared alum by these different processes with an advantage of more than 25 per. cent. over that of commerce, and that, notwithstanding the price to which it has now fallen, he still derives from 10 to 12 per. cent. profit. Lastly, the manufacturers of prussian blue, to whom the potash would be no expence, would at this time gain from 17 to 18 per. cent. by making this salt artificially.

Profits from these processes (in France.)

From the contents of this report, it is seen that the memoir of Citizen Curaudau contains the results of experiments made on a large scale, highly interesting to manufacturers and merchants, and that, in this view, it will be useful to circulate them through the channel of the scientific Journals. We are therefore of opinion, that the class will give much credit to

Citizen Curaudau for having published the result of his observations, and that it ought to encourage him thus to turn the product of his useful labours to the advantage of his country.

Done in the class of physical and mathematical sciences, the 11th Fructidor, in the year IX.

Signed, GUYTON and VAUQUELIN.

The class approves the report and adopts the conclusions.

XIV.

Letter from Mr. JOHN FARCY, concerning the great Fiery Meteor of Nov. 13, and on other Subjects.

To Mr. NICHOLSON.

SIR,

Correct date of the appearance of the fiery meteor in November last.
It was on the 13th.

THE great value of your Journal, as a record of facts connected with science, induces me to request you will correct the errors which have unfortunately crept into your account of the meteor of the 13th ult. which is described, in the second line of the title, and in the first line of the account at page 279, to have happened on the *fifth*; and also in the title of the cuts in the next page, where it is described as an event of the *sixteenth*. I am much concerned that your friend who saw this interesting phenomenon over St. Ann's church-yard, did not give an estimate of its elevation, or of its nearest approach to the zenith, or of the number of seconds during which the light occasioned by it continued.

Inquiries about its course, &c.

I was not so fortunate as to see it myself, or since to meet with any scientific person who did, but on the following evening, I enquired minutely of four or five watchmen of St. Martin's and St. Margaret's parishes, who were in different places at the time in their way to their respective watch-houses; I went with each of these persons to the spot, and heard their relation of every circumstance which they saw; one of these persons being just by the Horse-Guards within the Park, and another in the front of the Admiralty, agree in fixing the time at about twenty-nine minutes past eight by the Horse-Guards clock; from their comparison of its height with the seven stars then visible (though it was rather hazy) I am inclined to think that

that it passed within about 30° of the zenith, to the South of it, and in a direction from E S E. to W N W. by causing these persons separately to count one, two, three, &c. as long as they thought the light lasted, while I looked at my watch, I conclude its light lasted but five seconds, which agrees very well with the two or three seconds in which it was strong enough to affect persons in a room, as mentioned by you at page 279.

Greatest altitude about 60° , and course W.S.W.

During the very heavy fall of snow on Monday evening the 5th Inst. as I was returning home from Drury-Lane Theatre, I saw at distant intervals, four or five sudden flashes of light, which attracted the notice of many persons near me, and was concluded to be lightning: has lightning been observed during the formation of snow in the atmosphere? The air felt rather warm at the time.

Flashes of light during a fall of snow.

Your candour will, I trust, excuse me in lamenting the too concise descriptions given, at pages 218 and 219, of the syphon, and of the substitute for a fly wheel; it has not appeared evident to me that the syphon would act at all, without a stop cock below the funnel, *Fig. 3*. In line 7 of page 219 the print * has evidently made some mistake, and what is given below in explanation of *Fig. 2*, is so far from evident, that I hope you will at your leisure give a more detailed account. In page 220, last line but one, it should refer to *Fig. 4* instead of 3.

Observations upon some articles in this Journal.

You will greatly oblige me by giving a place in your Journal to a paper of Mr. Playfair's in the *Edin. Trans.* vol. V. 1802, containing Theorems on the Figure of the Earth. And to a paper in the *Trans. of the Linnean Society*, describing an extraordinary appearance of the layers of flint in the chalk strata of the Isle of Wight. I am in haste,

Valuable works indicated.

Sir,

Your obedient humble Servant,

JOHN FARCY.

No. 12, Crown-Street, Westminster,

Dec. 15, 1803.

REPLY. W. N.

Having received Mr. Woulfe's communication late in the month, I gave the account nearly in the concise words of the

F 2

inventor.

inventor. I will take an early opportunity of speaking more fully respecting it. In Mr. Howard's syphon the funnel part may in some measure be considered as a substitute for the side suction pipe. In large syphons neither will operate unless there be a stop cock at the bottom of the outer leg: in small instruments, such as that in the drawing, the stoppage is made by the finger.

The principal intention of a periodical journal being to publish in the form of a book such essays as from their conciseness might neither be circulated nor preserved adequately to their merits, it seems to follow that larger works must for the most part be exceptionable from the space they occupy. The excellent treatise of Professor Playfair being in English, and of considerable length, has not yet been reprinted in our work, chiefly for the latter reason; but I will avail myself of the obliging indications of Mr. Farcy to re-consider both the works he mentions.

XV.

Method of restoring rancid Essential Oils. By J. B. DE ROOVER.*

Process for purifying rancid volatile oils.

DURING my long and frequent experiments on essential oils, I have discovered a method of restoring them, when rancid, which appears to possess great advantages. The experiment which led me to it is as follows:—

One sixteenth of ether added to the oil, and left in digestion; five pints of alcohol added. Distillation on the water bath. Most of the oil remained behind.

I mixed one ounce of sulphuric ether with sixteen ounces of oil of peppermint slightly rancid; I left this compound in digestion for twenty-four hours, and then added five pints of spirits of the same mint. The solution was complete. I filtered the liquor and distilled it over the water bath. I expected that nearly the whole of the oil would have passed over in the distillation along with the alcohol and the ether; but, on opening the still, I was much surprized to find the greater part of the oil swimming on the surface of the residual fluid. To ascertain the quantity of oil which had passed over, I precipi-

* From the Journal of Van Mons (to whom it was communicated by the author,) V. 107. No. 13.

tated it from its solution in alcohol by water, according to my method, and four ounces and a half of oil were separated as transparent and colourless as water.

Oil was separated from the alcohol by adding water.

I then poured the residuum of the still, together with the spirit and the water of precipitation, after having separated the oil, into an apparatus over a naked fire, and distilled a second time. I had the satisfaction to find a spirit rise, the water of which, that came over at the same time, deposited an oil as perfect as before. I repeated this cohobation by pouring the mixture of spirits and water on the remaining oil through the neck of the alembic until I had collected in all twelve ounces and a half of oil as fragrant and free from rancidity as the freshest and best oil of peppermint.

Second distillation of all the products.

Cohobation and re-distillations,

I rectified the spirits and judged by some trials that it might contain rather more than one ounce and a half of oil.

Some oil in the alcohol.

What remained in the still was a thick adhesive matter of the nature of turpentine. I boiled it with water, and obtained ten gros (drams) of a sort of dry turpentine similar to resin.

Resinous matter in the still.

It is difficult to ascertain in what manner the addition of alcohol, or ether, contributes to the restoration, and in some measure the regeneration of so large a quantity of essential oil, which had lost the greatest part of its peculiar odour.

Theoretical speculation.

Formerly it would have been said, that the spiritus rector, with which the alcohol of peppermint was impregnated, had produced this effect; but now when we know that the smell of an essential oil does not depend on a peculiar principle, but on a constitution proper to the oily compound, we cannot admit that the oil of the alcohol had rendered any particle of the dis-
aromatized oil odorant; but we must suppose that the ether or the alcohol are in their decomposition identified with the substance of the oil.

In fact, the alcohol, and more especially the ether, contains as a constituent part hydrogen, which is the base of almost all the odours. Alcohol, ether, and the essential oils, are equally disposed to undergo an alteration in the relative proportion of their principles. There is consequently no obstacle to our admitting this concurrence of chemical operation for the restoration which I have obtained of the oil of *mint*.

Another circumstance tends to support this explanation. I at first attributed the oil which I obtained in the second operation to the difference of temperature of the bath, and of the naked

Theoretical speculation on the purifying of volatile oils.

naked fire. But the oily products of the subsequent distillations being all made at the same temperature, left me no cause for hesitation in explaining the effect by a successive re-action of the ether or the alcohol on the hydrogenated oil.

A third circumstance in confirmation of this opinion is, that the quantity of alcohol appeared to me to be sensibly diminished, and that in the smell of the oil, which was perfectly pure, I did not find the least trace of that of ether, which is so easily distinguishable. It is true that during the repeated distillations, the ether might have been dissipated as well as the portion of alcohol, which constituted the difference between the original and subsequent quantities of that liquid.

It remains, nevertheless, to be explained how a principle of the alcohol, by assimilation with the principles of the oil, should produce precisely the proportional compound which forms the species of oil on which it re-acts. To this it may be answered that the base of this oil is only disposed to take that principle which shall replace it in its original state.

But even supposing this explanation to be unfounded, and that the influence of the ether and the alcohol shall appear to be merely concurrent circumstances for producing a separation of the particles of oil and restoring its qualities, it is nevertheless certain, that the same effect has not hitherto been produced by any other means, and that my experiment furnishes us with a valuable method of restoring to commerce an immense quantity of essential oil, which would otherwise be of no value.

Second experiment.

P. S. Since the above mentioned experiment, I have made another trial with an oil which flowed as thick as turpentine, and had no smell by which the plant from which it was extracted could be distinguished. I mixed four ounces of this oil with two drams of sulphuric ether, and after four days distilled it with pure water. Two ounces and a half of oil came over as clear and as fluid as spirits of wine, having the best and most decided odour of wild thyme. There remained in the still two ounces of resinous matter of a dark red colour.

In this experiment the ether certainly developed the smell of the oil on which it re-acted, or rather it recomposed the oil by transmitting its hydrogenous principle to the oily base.

XVI.

Questions, 1. respecting the Place of the erect Image behind a Concave Speculum; 2. and of the Image formed by a Concavo-convex Mirror which is not Left-handed, and has the Property of revolving on its Axis along with the Mirror, but twice as fast; and 3. the Figure of the Sky. By a Correspondent.

To Mr. NICHOLSON,

S I R,

I HAVE a general knowledge of the delightful science of optics, but have no pretensions to an intimate acquaintance with its depths. I have therefore presumed to request you will either give your own sentiments on the following difficulties, or lay them before your readers, some of whom I hope will give a popular explanation of them. Questions proposed.

1. We are taught by all the writers on vision that the distance of any object is ascertained from the divergence of the rays from any assumed point on its surface, and that as the sense or judgment refers to the point of divergence, we may have a perception of an optical image by reference to that region or part of space from which the rays in their last course are directed, whether they have in absolute fact proceeded from this last point or not. Thus it is that we see an image behind a looking glass though the rays have never actually proceeded from that image; but only move in the same directions as if they had done so. If the mirror be convex, the image will be nearer to its surface, because the rays will be more scattered and divergent, and seem to have come from a nearer point. And parallel rays are said to come from an object infinitely distant. General statement of the means by which we infer the positions and distances of visible objects.

Now Sir, my query respecting the concave mirror is this:—When I hold my face close to the surface, the image and object touch each other. If I recede, the erect image recedes as the rays become less and less divergent. Every one knows that when I am at the distance of half the radius, the rays are returned parallel, and consequently the image is then infinitely distant. When I move farther off, the rays become convergent, and therefore in a certain sense may denote an image farther off than at an infinite distance; but I have no wish to discuss this point. When I am at the center of the sphere all is Phenomena of the images in a concave mirror.

confusion, and beyond that point I begin to discern the inverted image that hangs in the air, and is formed by the divergence of the rays from their focal interfections.

Apparent recess and approach of the erect image in a concave mirror.

Such are the facts, but not the whole of the facts. For I am very sure, that after the first receding of the erect image, it comes to a place where it is stationary, and then instead of remaining at any situation that may be called an infinite or more than infinite distance, *it comes forward again* and continues to advance till it is lost in confusion.

Difficulty stated. Why does the image advance?

Perhaps I may not have expressed myself with all the clearness I could wish; but I think I have shewn a difficulty and the reason why it puzzles me. The true science of nature must be adequate to the explanation of all phenomena, or at least it must not be contrary to any one of them. What is the solution of this?

Optical mirror at Merlin's.

2. There is an optical mirror at Merlin's Exhibition in Hanover-street, in which you see your face perpetually changing from one distortion to another, arising from its greatest length varying through all its several diameters. So that the face becomes absurdly long, crooked, broad, &c. If you move farther off, the face has its natural aspect, but is seen turning round its center and successively assuming the erect and inverted positions, and every intermediate degree of obliquity.

Singular distortions,

and revolution of the image.

A polished button of the same figure.

While these phenomena, produced by an apparatus concealed in a box, employed my occasional reflections, I happened to see a polished metallic button in the shops, which upon examination proves to be a mirror of the same kind as the larger one used at Merlin's. If it be considered in the direction of one of its diameters, it is a concave speculum; but in the direction of another diameter at right angles to the former, it is convex: and the radius of each curvature is about one inch. It is the same figure as a turner might produce in the lathe by using a chisel with a circular edge of one inch radius to scoop out a portion of a revolving piece till its smallest diameter, or calliper, became equal to two inches. Its effects are,

Particular description of the images produced by the button.

When the eye is brought very near the polished surface, it appears elongated in the direction of the diameter or chord, or of the concavity, and shortened in the diameter at right angles to that diameter, and if the speculum be turned on its axis, the image is affected with the strange undulation before mentioned, and undergoes all the variations twice in one revolution

Undulation.

volution of the mirror. But when the eye is at a considerable distance, a small perfect image is seen, which when the diameter of concavity is horizontal, is erect but not left handed; that is to say, if you move your right hand, the image also moves its right hand, instead of the left, as happens in a convex and a plain mirror; when the diameter of concavity is vertical, the image is inverted, but still right handed; and in the other positions of the mirror it takes every possible situation of obliquity, making two complete revolutions for one of the mirror itself. The image is larger the nearer the object, and the erect image is a little broader than the inverted; this deformity being more considerable the larger the image.

Small revolving image, not left-handed.

Now Sir, my simple question is, *Where is this image?* Is it before the mirror, according to the property of the convex, or behind it, according to that of the concave. I thought to have discovered this by the solar focus, or by applying a magnifier to the small image, but neither method answered my purpose, and nothing within the small extent of my knowledge has enabled me to solve it by reasoning.

Where is this image? before or behind the mirror.

3. The enlarged appearance of the moon near the horizon, and the flat vault presented by the concavity of the heavens, have been explained by various allusions to the diminished light after passing through a portion of the atmosphere, and its direction over a long row of objects, such as trees, houses, &c. Something seems to be wanting in these accounts. Neither the sun or moon are ever supposed to be more distant or larger when seen at considerable elevations through clouds or mists; and the sky seems flat, and these luminaries enlarged, whether the prospect be over woods, lawns, naked plains, or the calm sea. Besides which, I have no hesitation in insisting that the sky appears loftier on some days than on others, and even at the same instant of time on different sides. If this be no error, what are the causes of the appearance or the conviction produced in our minds?

Flatness of the sky and horizontal moon.

Not the same at all times, &c.

I am, Sir,

Your obliged reader from the first,

R. B.

I believe my correspondent would have vanquished his difficulties, with a little more consideration. If the explanation be not given by some of my correspondents, I shall endeavour to give the replies he requests in our next.

W. N.

A New

XVII.

*A New Method of preparing the pure Gallic Acid.**By M. L. SCHNAUBERT *.*Preparation of
gallic acid.

FOUR ounces of finely powdered galls are infused in water slightly alkalized with potash; the infusion which is of a very deep colour is filtered, and a solution of nitrate of tin is dropped into it, until no more precipitate is formed. The free acid of the liquid is then saturated with potash, taking care not to add the alkali in excess, and the precipitate is separated by filtration. The filtered liquid is then precipitated by acetite of lead, the weight and concentration of which is known. A greyish white precipitate is thus obtained, which is to be digested with diluted sulphuric acid, the proportion of the acid contained in which, must be to the concrete acetite of lead as one to four. After a sufficient digestion, the liquid is carefully filtered and evaporated, in order to afford crystals. If the gallic acid thus obtained, should contain a small quantity of sulphuric acid, it may be deprived of it by digestion, with gallate of lead.

In order to be certain of the quantity of sulphuric acid necessary for the decomposition of a given quantity of gallate of lead, I previously dissolved four ounces of acetite of lead in sixteen ounces of water, and diluted one ounce of sulphuric acid in four ounces of water. By mixing these two liquids to afford the perfect precipitation, the requisite proportion was easily found. I must here observe, that it will be better to keep the gallic acid in a liquid state than to reduce it to crystals, because evaporation always renders it more or less brown, I am firmly persuaded that the gallic acid possesses the property of becoming brown by the action of light, at least, if in contact with the atmosphere. For if gallic acid be sublimed perfectly white, and then dissolved in water, it will be found after some time to have undergone this kind of alteration. M. Bucholt has made a similar remark relative to the same fact.

* Tromsderff's Journal der Pharmacie, vol. ii. p. 61.

SCIENTIFIC NEWS.

Reward of Twenty Pounds for the Artificial Production of Palladium.

THE following is a copy of a paper received by me under cover, by the two-penny post. It is written in the same hand as a note which covered a small piece of the palladium mentioned to have been received by me last Midsummer. (See *Philos. Journal*, June, 1803. Vol. v. p. 136.) Upon inquiry, I find, that Mrs. Forster has received the sum of 20 £. with instructions conformable to this paper. This original is cut indent-wise on the margin, and has part of a manuscript flourish or paraph on each border, but no signature.

(COPY.)

December 16, 1803.

SIR,

AS I see it said in one of your Journals, that the new metal I have called palladium, is not a new noble metal, as I have said it is, but an imposition and a compound of platina and quicksilver, I hope you will do me justice in your next, and tell your readers I promise a reward of 20 £. now in Mrs. Forster's hands, to any one that will make only 20 grains of real palladium, before any three gentlemen chymist's you please to name, yourself one if you like.

It is insisted that palladium cannot be formed by art; and a reward is offered for any process to that effect,

That he may have plenty of his ingredients, let him use 20 times as much quicksilver, 20 times as much platina, and in short of any thing else he pleases to use: neither he nor I can make a single grain.

Pray be careful in trying what it is he makes, for the mistake must happen by not trying it rightly.

My reason for not saying where it was found, was, that I might make some advantage of it, as I have a right to do.

If you think fit to publish this, I beg you to give the names of the umpires, as I have desired Mrs. Forster to keep the money till next Midsummer, and to deliver it only in case they can assure her that the real metal is made by a certificate signed by you, and by them, on this check.

I hope a little bit of whatever is made may be left with Mrs. Forster.

Letter

Letter from Lalande on the Measure of a Degree of the Earth in Lapland.*

The measure of a degree in Lapland by Maupertuis, &c. suspected of error. ASTRONOMERS have long suspected, that there was some error in the measure of a degree of the earth taken in 1736 in Lapland, by Maupertuis, Lemonnier, Outhier,* and Celsius, because this measure was greater than it ought to have proved according to all others. Mr. Melanderhielm

Re-measured lately by Swanberg, &c. who found it to be 57209 toises, or 196 less. has had the good fortune to obtain a repetition of this measurement. He informs me, that Mr. Swanberg, and three other Swedish astronomers, have found the degree to measure 57209 toises in the latitude of $66^{\circ} 20'$, which are 196 toises

The flattening of the earth $\frac{1}{313}$ th. less than by the French measurement, and gives for the flattening of the earth $\frac{1}{313}$. This agrees better with the other comparisons, and teaches us, that the figure of the earth is not so irregular as was supposed, from the degree measured in the north.

Mechain gone to measure a triangle terminating at the balearic isles. Mr. Mechain set off on the 26th of April for Spain, whether he is gone to measure a triangle of 93000 toises, terminating at the Balearic islands, which will complete the grand and important measurement of the meridian, executed a few years ago by Messrs. Mechain and Delambre. He is accompanied by Messrs. Le Chevalier and Dezauche, and his youngest son. This measurement is very difficult, but no person is more capable of surmounting difficulties than Mr. Mechain, and we shall have the 54th degree in the middle of the whole arc measured by Frenchmen.

New metal. DESCOTILS has discovered a new metal in the ore of platina, it is presumed that palladium may be an alloy of this new metal with mercury.

A blue equal to ultramarine. Thenard has discovered a blue equally beautiful, and as fine as that of lapis lazuli, or ultramarine.

Prize of galvanism deferred. The National Institute has declared, that the galvanic prize will not be given this year.

These three articles are from the Journal of Van-Mons.

* Journal de Physique, May, 1803, p. 400.

Letter

*Letter from Professor PROUST to J. C. Delam  therie on a dangerous fulminating Powder *.*

IN one of my late lectures, I made an experiment that might have been attended with serious consequences to some of my auditors. Dangerous experiment.

It is, perhaps, no exaggeration to say, that the mixture of oxygenated muriate with arsenic, takes fire with the rapidity of lightning.

I am accustomed to compare the quickness with which different kinds of powder burn, by setting fire to them in cannons of equal diameter, passing each through a piece of cork placed in water, so as to sink in it almost their whole length. Mode of comparing explosions with that of gun-powder.

In this manner I was desirous of burning the arsenical mixture, by the side of an equal quantity of gun-powder, in order to compare their flame and continuance; but I had scarcely time to draw back a little after setting fire to the two cannons, when the first exploded and burst, breaking the glass vessel, and throwing about the water in all the radii of the surrounding sphere. Though the mouth of the tube and the escape of the flame were perfectly free, the cannon was burst from top to bottom, and spread out as flat as a card. A mixture of oxygenated muriate and arsenic burnt with the rapidity of lightning, burst the cannon that contained it, rendered it quite flat, and threw about the water in which it floated in all directions.

This powder is so violent in its effects, that I conceive it would be very dangerous, to attempt to make any use of it. Its effects are too dangerous for use.

If two long trains be made on a table, one of gun-powder the other of this mixture, and they be in contact with each other at one end, so as to be fired at the same time, you will see with great surprize, that one disappears like a flash of lightning, while the other seems to burn with extreme slowness. A long train of it disappears like lightning.

Letter from a Correspondent on the Expediency of converting Foreign Weights and Measures into English.

To Mr. NICHOLSON.

SIR,

THE pleasure which your readers receive from the perusal of your Journal is so much abated by the obscurity arising from the names of the French weights and measures, that I

Journal de Physique, May, 1803. p. 394,

with

wish to suggest to your consideration, that a reduction of them to English terms, would be to you comparatively a small labour, in proportion to what it would be to the generality of your readers individually.

Your labours tend to the useful purpose of communicating science and general knowledge to the public of this country. To facilitate the acquirement of such knowledge, it should be communicated in the terms most easily intelligible to the people; not in such terms as require algebra, or a process in the Rule of Three, to give a precise idea of every measurement. As your Journal becomes more extensively circulated, there arises a stronger claim upon you from the public, in favour of the many who are too indolent, too busy, or too ignorant to expound for themselves this species of hieroglyphics.

I am, Sir,

Your constant READER.

ANSWER.

The rule proposed to be followed in all our translations, though perhaps not precisely, and on every occasion, adhered to, is, that wherever the foreign denominations of weight or measure are mere indications of the proportional quantities, (and may be considered as pounds, ounces, or grains, or as yards, feet, or inches all through,) the original names are used without embarrassing the subject with any reduction into fractional quantities: In every other case the reduction is, or ought to be made.

While I express my acknowledgments to this reader for his advice and remarks, I beg leave to say, that I heartily subscribe to his opinions as to facilitating the communication of knowledge, by the simplest methods. It is my wish in the side notes, and by every other means, to shorten the time, and assist the inquiries, of those whose other duties forbid the indulgence of deep or continued researches.

W. N.

ACCOUNT

ACCOUNT OF NEW BOOKS.

A System of Theoretical and Practical Chemistry, in Two Volumes with Plates. By FREDERICK ACCUM, Teacher of Practical Chemistry, Pharmacy and Mineralogy, and Chemical Operator to the Royal Institution of Great-Britain. Octavo, Two Volumes, 733. and full Index and Contents, with Five Copper Plates. Accum's system of chemistry.

THIS work which, is dedicated by permission to the managers of the Royal Institution, and patronized by a very respectable list of subscribers, is drawn up for the use of such as are unacquainted with chemical science. The author, therefore, begins by describing such experiments as are best adapted to exhibit the general nature of chemical action. The laws of affinity, and also those which govern the phenomena of heat and light, are stated by the method of synthesis, and illustrated by experiment. And these are succeeded by the classification of bodies; namely, first the principles undecomposed, then the characteristic properties of the gases, and these are succeeded by the metals, alcalis, earths, &c. The display of these, as to their properties and habitudes, by an orderly combination of general narrative and reasoning, with particular facts, experiments or, operative processes, constitute the business of the present work, which will doubtless prove acceptable to the numerous cultivators of the science of chemistry.

A practical Essay on the Analysis of Minerals, exemplifying the best Methods of analysing Ores, Earths, Stones, Inflammable Fossils, and Mineral Substances in general. By FREDERICK ACCUM, Teacher of Practical Chemistry, Pharmacy, and Mineralogy. London, 12mo. 183 pages. 1804. Accum's analysis of minerals.

THIS active and indefatigable lecturer, has been requested by his pupils and auditors, to draw up a set of concise directions, to enable any person not intimately acquainted with analytical chemistry, to examine such unknown minerals as he may meet with, and readily ascertain their composition.

In

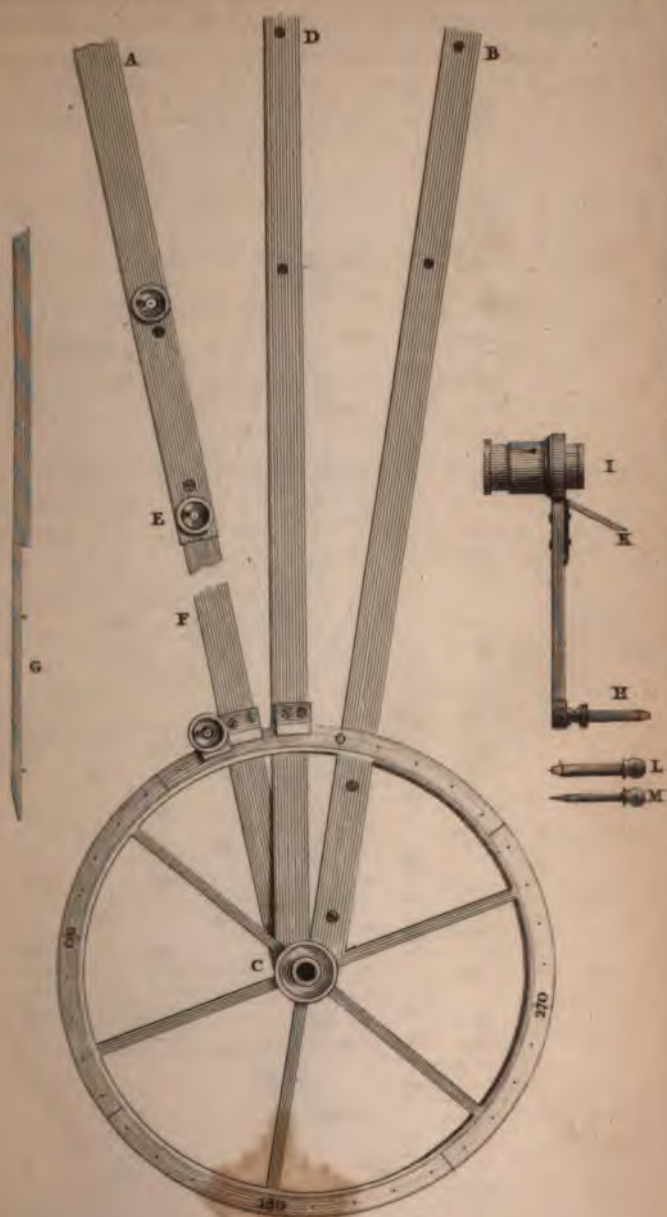
In the accomplishment of this object he has exceeded his first intention, and has not only given clear, and rather comprehensive instructions for analysing all the different genera, and principal species of the bodies mentioned in the title, and commonly met with, but has also given examples of analysis of the rarer or less abundant mineral bodies.

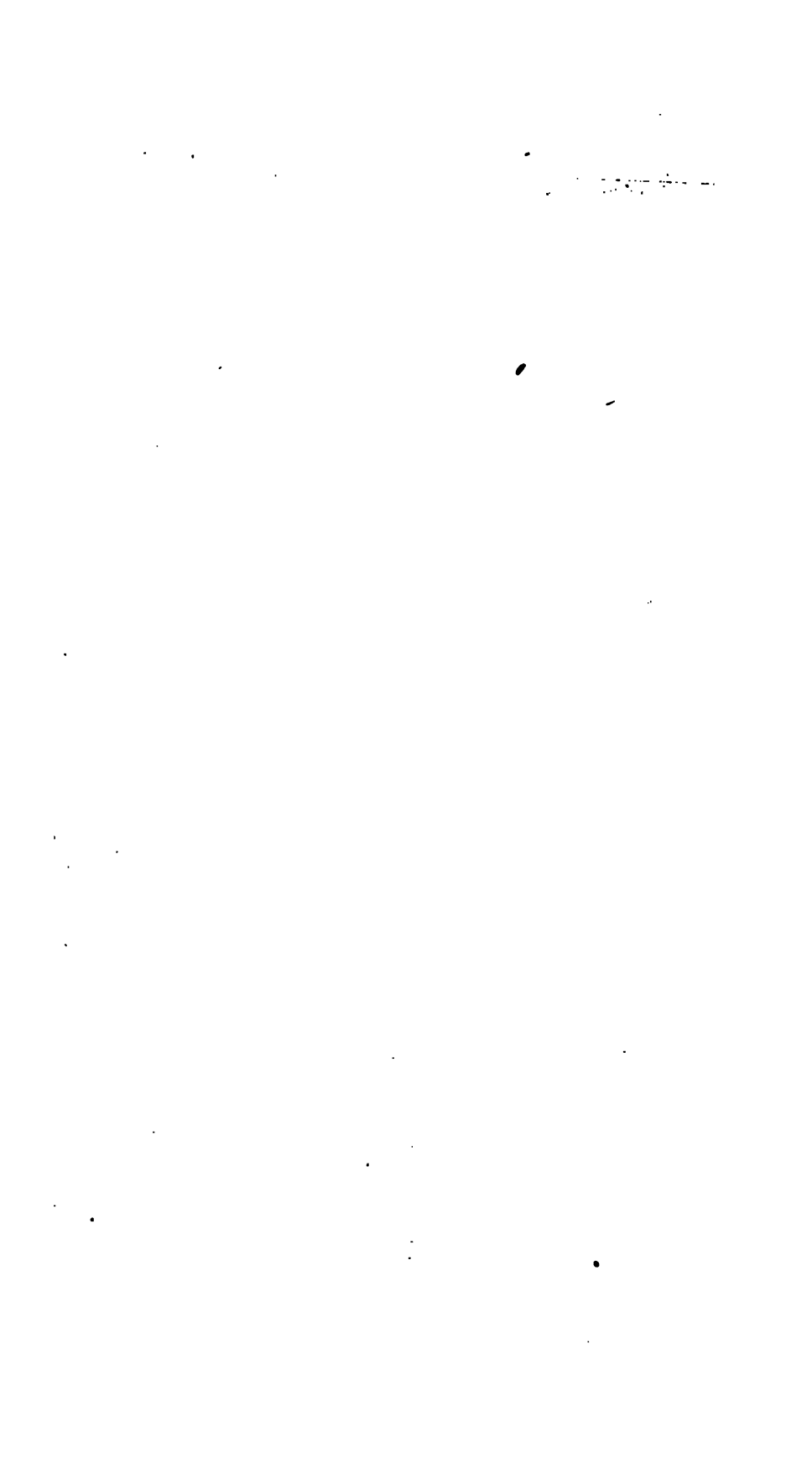
Bossut's history
of mathematics.

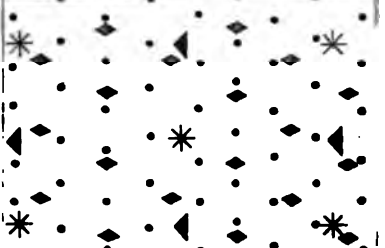
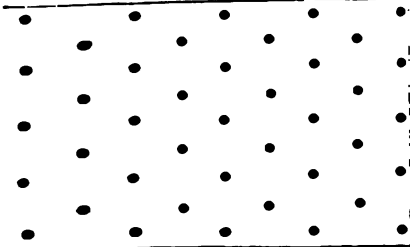
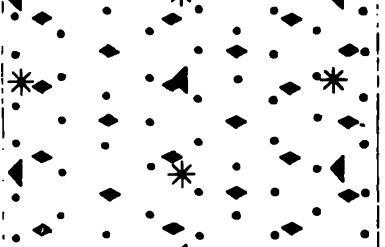
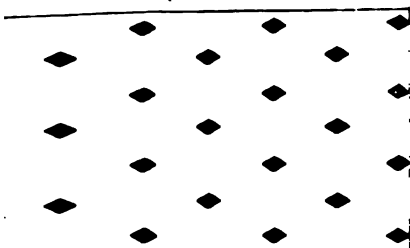
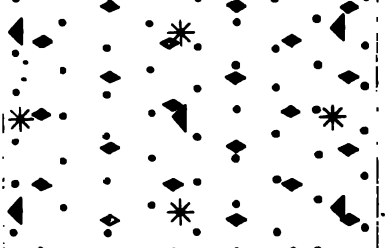
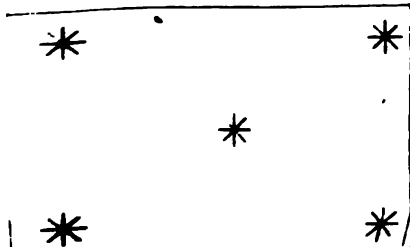
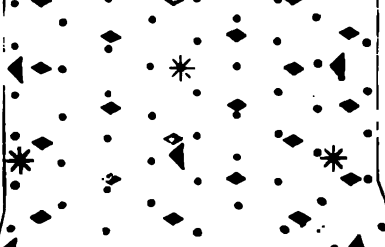
A General History of Mathematics, from the earliest Times, to the Middle of the Eighteenth Century. Translated from the French of John Bossut, Member of the French National Institute of Arts and Sciences, and of the Academies of Bologna, Petersburg, Turin, &c. To which is affixed, a Chronological Table of the most eminent Mathematicians. 8vo. 566. p. London, 1803. One Vol. Octavo.

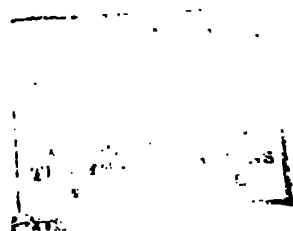
THIS justly esteemed work is well translated under the inspection of Mr. Bonnycastle, of the Royal Military Academy at Woolwich, who has added an introductory Preface, and the Table mentioned in the Title Page.

Station Pointer by Joseph Huddart Esq. F. R. S.





<i>M^r Dalton's Theory of the Gaseiform State.</i>	
<i>Carbonic acid Gas.</i>	<i>Compound. Atmosphere.</i>
	
<i>Hydrog. Gas.</i>	
	
<i>Oxygen Gas.</i>	
	
<i>Aqueous Vapour.</i>	
	



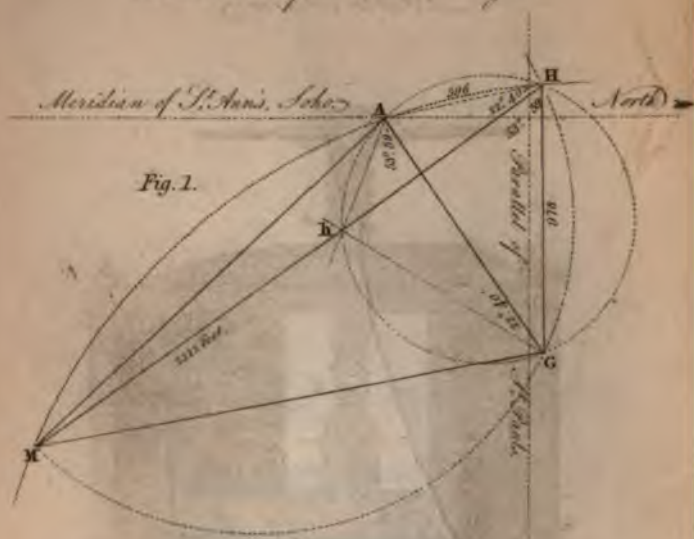
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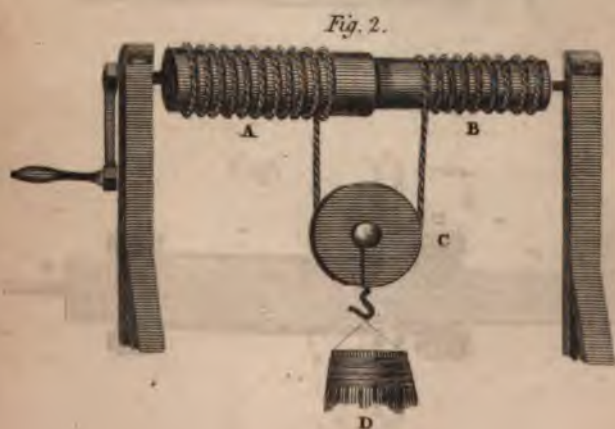
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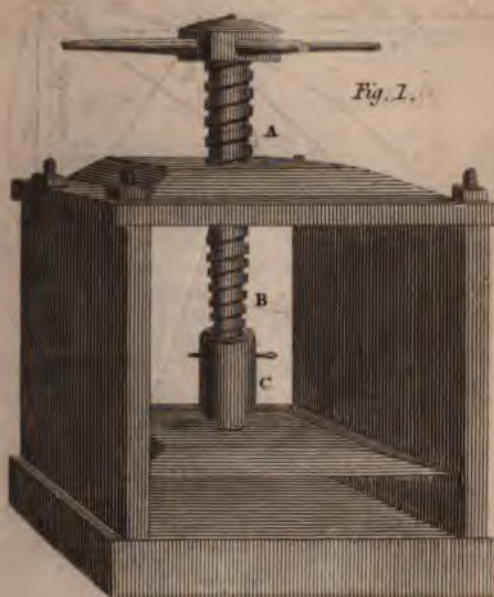
*Determination of Positions by
observation of three known objects*



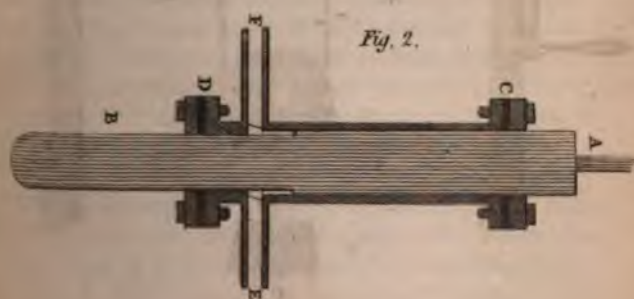
Winch & Barrell, (double)

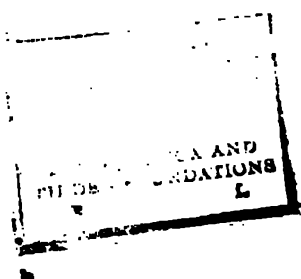


*Increase of power by a
Double Screw.*



Pump with double Piston.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1804.

ARTICLE I.

Description of an Apparatus, by which the Effect of Atmospheric Pressure in supplying Worm-Tubs and other Vessels with Water on the Syphon Principle, may be secured against any Interruption to be caused by the extricated Air. In a Letter from Sir A. N. EDELCRANTZ, Counsellor of the Chancery, and Private Secretary to the King of Sweden, Member of the Swedish Academy, &c. &c.

To Mr. NICHOLSON,

MY DEAR SIR,

ON my late return from an agricultural tour in Scotland, I find you have in your excellent Journal, honoured me with the mention of some ideas of mine, respecting the application of syphons to raise water, with less expence than usual, for coolers in distilleries, condensers of steam-engines, &c. The use of syphons is so well and so generally understood, that though the most obvious things do not always present themselves first to the mind, it would have been surprizing if the same notion had not likewise occurred to others. In fact, I have since learned, that a person in America, more than twenty years ago, obtained a patent for using the syphon to cool the worm-pipes in distilleries, and though I neither know his name, nor the specification of his invention, I presume it to be

Introduction:
respecting
former endeavours to supply worm-tubs, &c with water by the action of the syphon.

be similar to what I have proposed. In addition to which, the ingenious contrivance of Mr. Howard, published in a later number of your Journal, is also founded upon the same hydrostatical laws. But whatever may be the case in respect to theory, no practical use seems yet to have been made of it, owing most probably to unforeseen difficulties in the execution.

The chief impediment lies in the extrication of air which stops the current.

The principal of these, in my opinion, as was mentioned in the former memoir upon this subject, is the extrication of air from the water contained in the cooling vessel; which air being expanded, partly by its own elasticity under a diminished pressure, and partly by the heat, is accumulated in the upper part of the cooler, and interrupts the two columns of the syphon, so as at last to stop the motion entirely. For though it may possibly happen, by the negligence of workmen, that the worm itself may not be sufficiently sound to exclude the external air pressing into the cavity, and introducing itself that way into the cooler, as is stated in the experiment of Mr. Howard; yet I am persuaded that in most instances, this cause may be taken for the real one, instead of the former, which can never fail to act in proportion to the perpendicular height of the cooler, and the heat communicated by the worm. As it is impossible to observe by what actual means the air is produced, unless the whole apparatus were of glass, such a mistake may easily be made. In fact, if metallic pipes could not be made air-tight, even in a bended form, the exhaustions in air-pumps and still more in great condensations * would be attended with almost insuperable difficulties.

Not probable that this air comes from without.

Plan for extracting it.

For extracting the air disengaged from the water itself, during the action of the syphon, which in a few hours time would necessarily interrupt its motion, I have employed a few hours of leisure constructing the following means, which I beg leave, Sir, to communicate to you; without pretending to claim a greater portion of your attention than their remote utility may appear to deserve.

* In the experiments I made some years ago, with an apparatus constructed for condensing different gases and for producing a stronger artificial cold, than by any means employed before, I compressed air in brass pipes more than to $\frac{1}{2}$ of its original volume, which if we suppose the cooler to be 15 feet high, is more than 50 times the difference between the external and internal pressure of air in the cooling worm of the new apparatus.

On

On the top of the cooling vessel A, *Fig. 1. Plate V.* let a small vessel B, called the *air-vessel*, be placed, communicating with A by a cock. With the top of B let another vessel C, called the *water-vessel* be connected, having a cock C. These cocks have two parallel openings each, one for letting out air, the other for letting in water at the same time to take its place. Suppose these three vessels to be filled with water and the syphon in action, the cock B open and C shut; then all the air produced in the cooler will rise through B and fill the air-vessel; and in the mean time the water will descend. The air-vessel being filled with air, let the cock B be shut, and C opened; then the water from C will descend, and the air will escape into the atmosphere. By this alternate motion of the cocks, the air disengaged within the worm-tubs will be continually extracted, as long as any water remains in C.

The principal object then must be first to supply the water in C, either by manual labour, or by a part of the mechanical force employed to fill the artificial reservoir for the lower branch of the syphon; and secondly, to direct the alternative opening of the cocks, by a self-regulating mechanism; which may be effected in the following manner.

Fig. 2. Plate VI. represents a section of the *air-vessel* B, B is the air and water-cock, communicating with the worm-tub or cooler below. A is the other cock for the same purpose, communicating with the water-vessel C. A and B being connected by a metallic rod K L, their motions are simultaneous, and the perforations are so placed, that when the two holes of one cock are open, those of the other are always shut. E is a ball of lead or other weight, fixed on the lever G U, connected with the quadrant F G, and the lever G H, the whole turning through one quarter of a circle round the centre U; *m n* is a part of a cog-wheel fixed to and sliding along the quadrant G F, acting upon the teeth of the cock A, and forcing it, together with the cock B, one way or the other, as itself is moved by the levers U F or U G, either in the direction from F to G, or from G to F. This alternate motion is produced by the floater P O Q R, (of which a plan on a diminished scale, is represented in *Fig. 3.*) having in its middle and hollow part a kind of fork O, through which passes the metallic rod H I, connected with the lever H F, at the end of the quadrant G F. This floater in its ascension

A vessel filled with water, with a cock at top and another at bottom, is connected with the upper part of the worm-tub. When the lower cock only is opened the extricated air ascends, and water descends. When the upper cock only is opened, the air escapes, and other water fills in from above.

The upper water must be supplied; and the cocks must work without attendance.

The cocks are worked by a float: which when at lowest shuts the under and opens the upper cock; and when at highest does the contrary. The action is made sudden by a tumbling weight.

lifts the little ball H at the end of the lever, and consequently the weight E; and in descending it acts upon the ball I, and forces the weight E in the contrary direction. A side view of this apparatus is presented in *Fig. 4.* where the double openings in the cocks A, B, and the manner of fixing the weight E with its levers, are more distinctly seen.

In *Fig. 2.* the air-vessel is supposed half filled with air transmitted from the cooler. The cock B is shut, and the cock A open, admitting water in place of the air. The floater is ascending, and when the fork O touches the ball H, the weight E is lifted, the quadrant begins to move from E to G, but the sliding part *mn* remains in its place till the weight E, having passed the perpendicular and falling over the other side, brings the point F in contact with *n*, and forces at once both cocks in the contrary direction; by which A being shut and B open, the air from the cooler rises, and the water descends. The floater consequently sinks till it presses upon I, drawing the weight E in its first situation, and turning the cocks again. This process will evidently continue to alternate.

Every mechanical reader will see the necessity of turning the cocks in this apparatus by a sudden stroke. For if the floater were to act directly and constantly upon them; there would be a certain moment when both would be shut, and consequently the motion would cease.

Another method
by chains.

Instead of the slider *mn*, the same effect may be produced by two chains, (*Fig. 5.*) on the ends of the levers FG, connected with the cock A; the other apparatus remaining as described above. This method may even in some respects appear more simple; but I have described the other partly because I apprehend the contrivance to be new, and partly because the use of chains may in some cases be liable to objections.

An air-pump
would be the
simplest organ.

But I cannot avoid remarking that the most simple means of all, and perhaps the best, on a small scale, would be an *air-pump* fixed on the top of the cooler, and connected with the pump that supplies the artificial reservoir. And though the force required for extracting the rarified air, must be in proportion to the perpendicular height of the syphon; yet it cannot be more than what is required to raise a quantity of water necessary to fill the place of the air, as in the former

apparatus. When it has been found by experiments, how much air is extricated from a given quantity of water, running with a known velocity, under a given diminution of pressure and heat; the size and power of the air-pumps may be determined so as to act with full advantage, as long as these circumstances remain unvaried. But if they happen to change, the air-pump will either work too much, and expend an unnecessary force, by extracting water after the air is exhausted; or too little, by extracting the air more slowly than it is produced. On the contrary, it seems that the floater, if the vessels and cocks be of sufficient size, can never deliver more or less than is exactly required.

This consideration, as well as the possibility of a constant supply of water for small vessels, without any pumping at all, may in many cases allow the first apparatus to be used with advantage, and even render it preferable, especially in elevated syphons and great rarefactions, as most commonly occur in practice, when it would not only be very difficult to keep the air-pump tight without great friction, but a quantity of vapour from the heated fluid would be expanded. This increases the difficulty of the air-pump, but in the other contrivance it is partly condensed by the cold water in the air-vessel trough, which it is obliged to pass.

I am with great esteem, My dear Sir,

Your obedient humble Servant,

A. N. EDELCRANTZ.

II.

Enquiries concerning the Nature of a Metallic Substance lately sold in London as a new Metal, under the title of Palladium.

By RICHARD CHENEVIX, Esq. F. R. S. and M. R. I. A. *

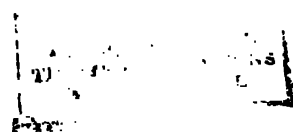
ON the 29th of April I learned, by a printed notice † sent to Mr. Knox, that a substance, which was announced as a new metal, Announce of a new metal for sale.

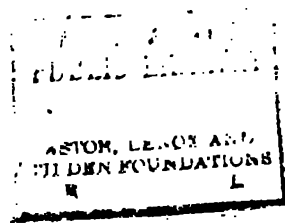
* From the Philos. Transf. 1803.

† "Palladium, or new silver, has these properties amongst others that shew it to be a new noble metal.

"1. It dissolves in pure spirit of nitre, and makes a dark-red solution. 2. Green vitriol throws it down in the state of a red gulus from this solution, as it always does gold from aqua regia.

"3. It





effect of heat upon that substance. Hence we may conclude, that there is not any action between charcoal and palladium.

Alloy. Palladium and gold; gray and less ductile.

I put equal parts of palladium and gold into a crucible, for the purpose of forming an alloy. The result, owing to an accident, did not weigh so much as the sum of the quantities employed; therefore, the proportions in this alloy were uncertain. Its colour was gray; its hardness about equal to that of wrought iron. It yielded to the hammer; but was less ductile than each metal separate, and broke by repeated percussions. Its fracture was coarse-grained, and bore marks of crystallization. Its specific gravity was 11,079.

Palladium and platina; denser, less malleable.

Equal parts of platina and palladium, entered into a fusion at a heat not much superior to that which was capable of fusing palladium alone. In colour and hardness, this alloy resembled the former; but it was rather less malleable. Its specific gravity, I found to be 15,141.

Palladium and silver; whiter.

Palladium, alloyed with an equal weight of silver, gave a button of the same colour as the preceding alloys. This was harder than silver, but not so hard as wrought iron; and its polished surface was somewhat like platina, but whiter. Its specific gravity was 11,290.

Palladium and copper; harder dull colour.

The alloy of equal parts of palladium and copper was a little more yellow than any of the preceding alloys, and broke more easily. It was harder than wrought iron; and, by the file, assumed rather a leaden colour. Specific gravity 10,392.

Palladium and lead; more fusible, gray, close grained, and hard and brittle.

Lead increases the fusibility of palladium. An alloy of these metals, but in unknown proportions, was of a gray colour, and its fracture was fine-grained. It was superior to all the former in hardness, but was extremely brittle. I found its specific gravity to be 12,000.

Palladium and tin; grayish, brittle, compact.

Equal parts of palladium and tin gave a grayish button, inferior in hardness to wrought iron, and extremely brittle. Its fracture was compact and fine-grained. Specific gravity 8,175.

Palladium and bismuth; gray, very hard and brittle.

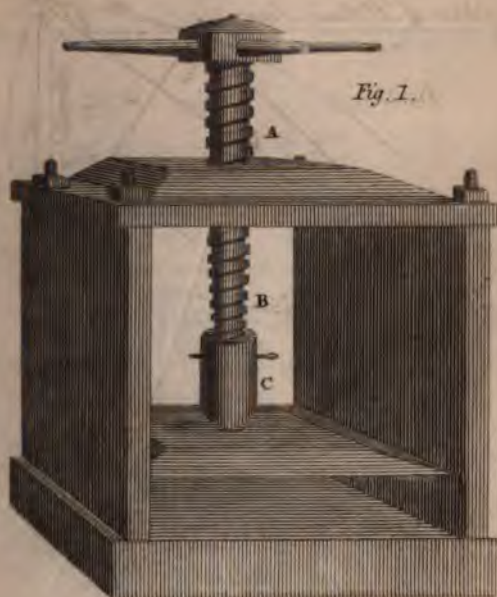
With an equal weight of bismuth, palladium gave a button still more brittle, and nearly as hard as steel. Its colour was gray; but, when reduced to powder, it was much darker. Its specific gravity, I found to be 12,587.

Palladium and iron. Palladium and arsenic.

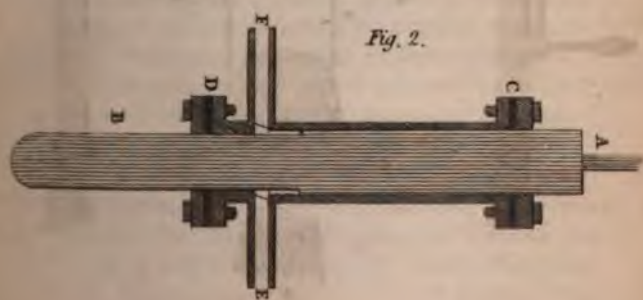
Iron, when alloyed with palladium, tends much to diminish its specific gravity, and renders it brittle. Arsenic increases the fusibility of palladium, and renders it extremely brittle.

From

*Increase of power by a
Double Screw.*



Pump with double Piston.



Sulphuric acid and palladium. Weak action: beautiful red solution.

Sulphuric acid, boiled upon palladium, acquires a beautiful red colour, and dissolves a portion of the substance. The action of this acid is not very powerful, and, upon the whole, it cannot be looked upon as a good solvent for palladium.

Nitric acid more active; fine red solution.

Nitric acid acts with much greater violence upon palladium. It oxidizes the substance with somewhat more difficulty than it can oxidize silver; and, by dissolving the oxide, forms a very beautiful red solution. If the nitric acid be impregnated with nitrous gas, its action upon palladium is much more rapid.

Muriatic acid, Red solution.

Muriatic acid, by being boiled upon palladium for a considerable time, acts upon it, and becomes of a beautiful red.

Nitro-muriatic acid is the true solvent.

But the true solvent of palladium is nitro-muriatic acid, which attacks it with great violence, and forms a beautiful red solution.

Precipitate from acids by alkalis or earths, fine orange partly redissolved.

From all these acid solutions of palladium, a precipitate may be produced by the alkalis and earths. These precipitates are, for the most part, of a beautiful orange; are partly redissolved by some of the alkalis; and the supernatant liquor of the precipitate formed by ammonia is sometimes of a fine greenish blue. Sulphate, nitrate, and muriate, of potash, or of ammonia, produce an orange precipitate in the salts of palladium, as in those of platina, when not in too dilute solution; and the precipitates from the nitrate of palladium are in general of a deeper orange. All the metals, except gold, platina, and silver, cause very copious precipitates in solutions of palladium. Recent muriate of tin produces a dark orange or brown precipitate in neutralized salts of palladium, and is an extremely delicate test. Green sulphate of iron precipitates palladium in the metallic state; and, if the experiment succeed, the precipitate is about equal in weight to the palladium employed. Prussiate of potash causes an olive-coloured precipitate; and water impregnated with sulphuretted hydrogen gas, a dark brown one. Fluoric, arsenic, phosphoric, oxalic, tartaric, citric, and some other acids, together with their salts, precipitate some of the solutions of palladium, and form various combinations with this substance.

Such are the principal characters I have found in palladium, examined as a simple metallic body. It does not appear that, in stating any of its properties, except its specific gravity, the printed notice has been guilty of misrepresentation.

From

From these experiments, it would be difficult to say of what metal, or of what combination of metals, palladium consists. We could not suppose gold or platina to be an ingredient in it, as it is in some measure acted upon by sulphuric and muriatic acids, and is wholly soluble in nitric acid. Silver is excluded, by the effect of muriatic acid upon its solutions; as is lead, by that of the sulphuric. Tin, antimony, bismuth, or tellurium, would have left an insoluble residuum with nitric acid. No traces could be found of any of the acidifiable metals; and iron was looked for with particular care, but in vain. In a word, the precipitation by the metals, seems to exclude all those of easier oxidability than mercury; and this we should not suppose to be present, as copper is not in the least whitened, when used to precipitate palladium.

Comparison of this body with the other known metals.

The striking similarity of many of the precipitates of palladium with those of platina, induced me to multiply the comparative experiments; and I constantly observed contradictory facts. The specific gravity, easy fusibility, combination with sulphur, precipitation by green sulphate of iron and by prussiate of potash, together with other effects, were such as I could not reconcile to the known characters of platina; unless I could suppose that a substance did exist, which could totally change its physical and chemical properties, or so disguise them as to render them proof against the evidence of chemical re-agents.

It resembles platina in many of its precipitates, &c.

The lightest of the metals is tellurium; yet, in order to produce an alloy of the specific gravity of palladium, (supposing for a moment the real density of the alloy equal to the calculated mean), it would require two parts of tellurium and one of platina; and it is highly improbable, that so large a proportion of tellurium could exist in any mass, without being detected. We have been told of very extraordinary anomalies in chemical affinities, by Mr. Berthollet; and Mr. Hatchett has made us acquainted with some, not less extraordinary, in the properties of alloys. Yet I think we shall cease to wonder at what has been related by these chemists, when we learn that palladium is not, as was shamefully announced, a new simple metal, but an alloy of platina; and that the substance which can thus mask the most characteristic properties of that metal, while it loses the greater number of its own, is mercury.

It is an alloy of platina and mercury.

I confess it was not from an analysis of palladium that I was first led to this result; for I had convinced myself, by synthesis,

This was synthetically discovered.

Admirable disposal of platina and mercury by which they were presented to each other and united.—Green sulphate of iron was added to a solution of platina; and also to a solution of mercury. No precipitate took place in either. The compounds were then mixed. Platina and mercury fell down together. The compound was fused. It was palladium.

Palladium by the immediate union of platina and mercury.

of its nature, and had formed the substance, before I could devise any probable method of ascertaining its component parts.

In reflecting upon the various modifications which substances undergo when in union with each other, and on the variations produced in the laws of affinity by the intervention of new bodies, I was induced to try whether by the affinity of platina with some metal easily reduced, it might not happen that a reduction of both would take place by green sulphate of iron, although no such effect were produced upon each metal when separate. The most likely to succeed, as being most easily reduced, after gold, platina, and silver, was mercury. I poured some solution of green sulphate of iron into a salt of platina, and also into a salt of mercury; no precipitation took place. I united the two liquors; and a precipitate exactly resembling that which is formed by green sulphate of iron in palladium, was instantly formed. I collected the precipitate, and exposed it to a strong heat; and, after repeated trials, obtained a metallic button, not to be distinguished from palladium.

It certainly is one of the most extraordinary facts respecting alloys, that two metals, by their union with each other, should so lose the characteristic properties of each individually, that neither of them can be immediately detected by the usual methods. Nothing but an affinity of the most powerful order could produce such effects. But, to place the metals under the most favourable circumstances for that affinity to exert its influence, and to promote their union, is not the result of common methods. Among a great number which I have tried, many have failed, and none have been attended with uniform success. I have, however, formed palladium by the immediate union of platina and mercury; and, as whatever may place the apparent capriciousness of this combination in a conspicuous point of view is not devoid of interest, I shall describe the means by which I have attempted to produce it, whether they failed, or were attended with success.

SYNTHETICAL EXPERIMENTS.

Exp. 1. It was not till after repeated trials of the mode just mentioned, that I succeeded in forming palladium. In many instances, I obtained a button completely melted, of the specific gravity of 13, and sometimes more; not so easily fused by sulphur as palladium; not soluble in nitric acid; and the absolute

absolute weight of which exceeded that of the platina originally employed. But, although this substance was not platina, I could not say it was palladium. The most successful experiment by this method, was attended with the following circumstances. I dissolved one hundred grains of platina in nitromuriatic acid, and then put in two hundred grains of red oxide of mercury, made by nitric acid; but this not being sufficient to saturate the excess of acid, I continued to add more, until it ceased to be dissolved. On the other hand, I prepared some green sulphate of iron, and poured it into a long-necked matrass. I then poured the mixed solution of platina and mercury into the solution of green sulphate of iron, and heated the whole upon a sand bath. In less than half an hour, a copious precipitate was formed; and the inside of the matrass was lined with a thin metallic coat. The liquor was passed through a filtre, which I had weighed; and the precipitate, after digestion with muriatic acid, was well washed and dried. When I had collected as much of this as I could, there remained upon the filtre 12 grains; besides which, I had collected 264, in all 276. The supernatant liquor still contained a portion of mercury, and about eight grains of platina. Therefore, the 276 were composed of 92 of platina, and 184 of mercury. From this it appears, that one hundred grains of platina, can determine the precipitation of near two hundred grains of mercury, by green sulphate of iron; and that, in this proportion, there is a reciprocity of saturation. The 264, collected from the filtre, were exposed to a low red heat, and were reduced to 144. The twelve of the filtre would have given about seven; therefore, the whole would have been 151. The substance was in the form of a fine powder, and had a metallic lustre. It was then put into a charcoal crucible, and fused into a button. This button weighed 128 grains, and with the quantity left on the fibre, would have weighed 135. In this 135, there were 92 of platina; therefore it was composed of about two parts of that metal and one of mercury. It was of the specific gravity of 11,2; was wholly soluble in nitric acid; was easily fused by sulphur; was precipitated by green sulphate of iron: in a word, it was not to be distinguished from palladium.

Experiment 1.
The most successful experiment of forming palladium by the mode first mentioned.

It contained about two parts platina and one mercury.

Exper. 2. As another mode of forming palladium in the same way, I put metallic iron into a mixed solution of platina

Experiment 2.
Mixed solution of platina and
and

mercury was precipitated by iron. The success was less complete.

and mercury. Both metals were precipitated; and the precipitate was submitted to the same treatment as in the former case; but the success was not so complete. Iron can precipitate either platina or mercury separately; but green sulphate of iron can perform its function only in favour of the affinity of platina and mercury. Their union is promoted by its action; and the effects are, in all probability, simultaneous. The combination of the metals takes place, if I may be allowed the expression, in their nascent metallic state, and in a fixed proportion of mutual saturation. The union of the two metals, therefore, is in the present experiment less intimate, and the button which results from fusing the precipitate, is of much greater density.

Exp. 3. Zinc the precipitant. Incomplete.

Exper. 3. The same process was repeated, only using zinc instead of iron, but the result was not more satisfactory.

Exp. 4. Mercury added to solution of platina. Incomplete.

Exper. 4. I poured some mercury into a solution of platina, and heated them together for some time. A precipitate took place; but, upon fusing it into a button, I did not find it to be palladium.

Exp. 5. Mixed solutions of platina and mercury evaporated and ignited. The mercury flew off.

Exper. 5. I dissolved the same quantities of platina and mercury as in *Exper. 1*, in nitro-muriatic acid, and evaporated those solutions together. I then volatilized as much as I could of the mercury, at a red heat. At the end of the operation, I obtained precisely my original quantity of platina, reduced to the metallic state; but not one particle of the mercury remained along with it.

Exp. 6, 7. Platina and mercury precipitated by phosphate of ammonia. No success.

Exper. 6 and 7. The same quantities of platina and mercury, dissolved in nitro-muriatic acid, were precipitated by phosphate of ammonia; and the liquor was evaporated. The residuum, in a glassy state, was exposed to a violent heat in a charcoal crucible; and I obtained a melted button, which weighed more than the original quantity of platina, and was of the specific gravity of 14.5. On account of the easy fusibility of phosphuret of platina, I likewise tried to combine it directly with mercury, but could not succeed.

Phosphuret of platina.

Exp. 8. Platina and mercury precipitated by sulphuretted hydrogen. In one instance palladium.

Exper. 8. I precipitated a mixed solution of platina and mercury, by a current of sulphuretted hydrogen gas; and reduced the insoluble powder. After many attempts, in which I obtained buttons of the specific gravity of 14.3 and 14.5, I formed a piece weighing 11 grains, of the specific gravity of 11.5. This

11,5. This last was palladium; but I could not ascertain the excess of weight, as a part of the original precipitate had been lost.

Exp. 9. I mixed a solution of muriate of platina with prussiate of mercury, and obtained a slight precipitate. The liquor was evaporated, and the whole residuum exposed to a violent heat. This experiment did not succeed. It was not repeated so often as the others; but I have some reason to think it might be attended with success; for I obtained, in one instance, a few very minute grains, that were soluble in nitric acid.

Exp. 9. Muriate of platina and prussiate of mercury. Evaporation and strong heat. Partial success.

Exp. 10. I heated some purified platina, in the form of a very fine powder, with ten times its weight of mercury, and rubbed them together for a long time. The result was, an amalgam of platina. This amalgam, exposed to a violent heat, lost all the mercury it had contained; and the original weight of the platina remained without increase.

Exp. 10. Amalgam platina and mercury. Heat drove off the mercury.

Exp. 11. The best method of forming an amalgam of platina, is that prescribed by Count Mussin Pushkin. I dissolved a known quantity of platina in nitro-muriatic acid, precipitated by ammonia, and evaporated the liquor. The residuum was rubbed for a long time with a great quantity of mercury, and then exposed to a violent heat. Many operations failed; in some, I had a button of the specific gravity of 13,2. In one attempt, I succeeded completely: from 30 grs. of platina, treated as above, I obtained a button weighing 43,5, of the specific gravity of 11,736, which had all the properties of palladium.

Exp. 11. Count Mussin Pushkin's amalgam of platina heated gave palladium in one attempt.

Exp. 12. I fused together, in a charcoal crucible, 100 grs. of platina, 200 of cinnabar, 100 of lime, and 400 of calcined borax; and obtained a button, which weighed more than the platina, and was of the specific gravity of 15,7. It was not soluble in nitric acid; but combined with sulphur, at a red heat.

Exp. 12. Platina, cinnabar, lime and borax fused.

Exp. 13. In some experiments I had made, I found that the furnace in which I formed these alloys, was capable of melting platina, without the assistance of any flux except calcined borax. I therefore urged 100 grs. of platina, at a very strong heat; and, when I judged the fire to have attained its greatest intensity, I poured mercury upon the platina, through a long earthen tube that terminated in the crucible,

Exp. 13. Platina fused and mercury thrown in. No effect.

and immediately withdrew the apparatus from the fire. No sensible union of the metals had taken place; nor had the platina increased in weight.

Exp. 14. Mercury in vapour passed over ignited platina. No effect.

Exp. 14. I put 100 grains of platina into an earthen tube, and placed the tube horizontally in the above furnace. At one end of it was a resort, containing 2lbs. of mercury. When the tube was at its greatest heat, the mercury was made to boil; and the entire quantity passed over the surface of the platina, at that temperature. The experiment lasted one hour and a half; but the metals did not seem to have combined.

Exp. 15. Platina wire plunged in mercury and heated by galvanism. Little effect.

Exp. 15. Mr. Pepys was so obliging as to try the effect of his very powerful galvanic battery, in forming palladium. A piece of platina-wire was plunged into a basin of mercury, and formed part of a galvanic circle. The wire was nearly in fusion; but no combination seemed to take place. The nature of this experiment did not allow of very accurate weighing; but the fused globules of platina did not appear to have acquired the properties that constitute palladium.

Remarks. Disposing affinities and assimilation.

Such are the experiments by which I attempted to form palladium. They were chiefly founded upon two principles; disposing affinity, and assimilation. In the one case, I endeavoured to present to the metals that compose it, a substance which, on account of its affinity for some menstruum necessary for their solution, and of their own tendency to combine in the proportions stated in *Exp. 1*, might cause them to unite in the form of an insoluble compound. In the other case, I hoped to assimilate the properties of each, and, by making them something more alike, to place them in the most favourable circumstances for uniting. *Exp. 1*. was founded on the former, and *Exp. 8* on the latter of these principles.

The platina always acquired weight when its properties were thus changed.

Mercury was indispensable.

In many instances, when I did not form palladium, I obtained a metallic button which was not platina; and, when I did so, it always weighed more than the original quantity of platina employed. In repeating *Experiments 1, 2, 4, 6, 8, 11, and 12*, I seldom failed of having such a substance. No effect of this kind took place in any experiment, when mercury was not used along with platina; and the other metals were merely accessaries, in promoting their union and precipitation. This is sufficiently proved, by the uniformity of the results in different processes, whether it was palladium or the substance I now mention which was formed. The chief property which distinguishes

distinguishes the latter substance from platina, is its density. It is not unusual to obtain it of a specific gravity so low as 13; very frequently 15 or 17. In the first experiments, I suspected this lightness to be owing to some air-bubbles; but repeated fusion, and comparative experiments upon platina, soon convinced me of the contrary. The augmentation of weight also, which the platina never fails of acquiring, proves that this metal has combined with some ponderable substance; and, in fact, the result of these operations is, an alloy which is a mean betwixt platina in its pure state and what has been called palladium. It is, consequently, subject to infinite variation. The first effects which mercury produces upon platina are, to render it more fusible, and to diminish its specific gravity. The next new property conferred upon it is, the power of uniting with sulphur; and, lastly, it becomes soluble in nitric acid. It is not however till the specific gravity is below 12, or 12,5 at most, that it has acquired this property; and all these effects follow the direct order of the increase of weight observable in the platina.

It is not very difficult to combine a small quantity of mercury with platina; but, to resolve the problem completely, and to produce an alloy of these metals which shall be of so low a specific gravity as 11,3, and shall be soluble in nitric acid, is not so easily accomplished. From the repeated failures which I have experienced in these operations, I am much inclined to think that the author of palladium has some method of forming it, less subject to error than any I have mentioned. No doubt that perseverance would put us in possession of his secret; but, being prevented by want of leisure from pursuing these researches at present, I have confined myself to establishing the fact, and describing the processes which I have employed.

Having thus acquired a certainty that mercury is a constituent part of palladium, I made some further experiments upon it, with a view to its analysis; but they have not been attended with so much success. It might be expected, from the great number of methods which have failed to form palladium, that many might be found to decompose it when formed. But I have found the converse of such processes as did not succeed in producing palladium, to be ineffectual in destroying the combination.

Mercury first renders platina more fusible and lighter; then renders it combinable with sulphur; and still more renders it soluble in nitric acid, viz. under the specific gravity 12.

The author of palladium probably has some steady method of composing it.

Its decomposition extremely difficult.

ANALYTICAL EXPERIMENTS.

Analytical experiments not successful. Excess of mercury added to solution of palladium.

Exp. 1, 2, and 3. The converse of the synthetical experiments 1, 2, 3, was made, but without any satisfactory result.

Exp. 4. The converse of *Exp. 4* was made without success. I put some mercury into a solution of palladium, and left them together for some time. The precipitate which was formed was palladium, just as it had been used for the operation.

Palladium strongly and durably heated.

Exp. 5. I exposed different pieces of palladium to a very violent heat for two hours. In some, a diminution of absolute weight, with an increase of specific gravity, took place; in others, neither of these effects was produced. The specimens which I had made were chiefly of the latter kind.

Cupellation.

Exp. 6. Cupellation did not afford any satisfaction respecting the analysis of palladium; but the heat necessary for this purpose is so great, that I could not place great reliance upon this experiment. It is difficult to detach the button from the cupel with accuracy.

Combustion in oxygen,

Exp. 7. I burned some palladium in oxygen gas. A white smoke arose during the combustion, and was deposited upon the sides of the glass jar that contained the gas. But this smoke was palladium, and not the mercury separated from it.

by galvanism.

Exp. 8. A slip of palladium, which Mr. Davy had the goodness to expose, in my presence, to the action of the strong galvanic batteries of the Royal Institution, burned with a very vivid light, and a white smoke; but no mercury was separated by this operation.

Wonderful differences between mixture and combination.

Affinity not proportioned to the facility of union.

There is not any property of this compound which appears to me so wonderful, as that which is manifested by these experiments. It is a striking proof how unfounded was the opinion of some philosophers, who supposed that the rapidity of combination was a measure of the force of affinity. We do not know of any affinity among chemical bodies which is more powerful than that of platina and mercury appears to be. The obstacles which must be overcome, in order to fix the latter metal, are a proof of this; yet the difficulty of forming this combination to its full extent is extreme. The difference which exists between the compound and its elements, when merely mixed, either in solution or otherwise, cannot be better

better exemplified than by comparing the result of the 5th synthetical experiment, with the difficulty of expelling mercury from the compound.

I must here observe, that all the analytical experiments, and many others, were made, by way of comparison, upon the palladium I had bought, as well as upon that which I had made. But, although I had myself combined the mercury with the platina, and consequently knew it to be in the compound that resulted, I could not succeed in separating it. Neither did the substance described in a former paragraph, as intermediate between platina and palladium, allow one particle of mercury to escape from it, by any process I have yet been able to devise.

The mercury could not be separated from the bought palladium; nor from any of the author's real combinations.

The name of palladium conveys to our mind the idea of something absolute, and therefore incapable of gradation. But gradations in alloys are infinite; and the alloy of platina and mercury is susceptible of infinite variation. Palladium also brings to our recollection a contemptible fraud directed against science: the name, therefore, should not be admitted. I have called it an alloy; for it differs too much from the usual idea we have annexed to the word amalgam, but it accurately corresponds with our notions of the name I have adopted.

Objections to the word palladium.

The facts which I have related in this paper, appear at first sight to have no similar examples in chemistry; and may not gain immediate assent from every person. The philosopher, indeed, will feel no humiliation in being forced to correct or to extend his knowledge; and will not altogether disbelieve a fact, because he can adduce no parallel instance, or because it is not in unison with his received opinions. Such conduct would be raising an insurmountable barrier against the progress of science; it would be setting up our own feelings in the place of nature; and attempting to measure what in itself is immeasurable, by the narrow scale of human comprehension.

These new facts are worthy of entire credit.

But let us not confine our view of the facts and principles that have been mentioned, to this single instance. Let us trace them in a more extended circle; and see whether any thing may be found in nature that can apply to the present subject.

The first prejudice, for such I must call it, against the presence of platina in palladium is, the small density of the alloy. And no doubt it is extraordinary, that a metal the specific gravity of which is at least 22, (Chabaneau says 24,) combined with another the specific gravity of which is nearly 14,

The strange diminution of specific gravity in this compound must not prejudice our belief.

Other similar facts.

should produce a mass of the specific gravity of 10,972; not much more than half of that which calculation would denote, and inferior to either of its elements. In Mr. Hatchett's Paper upon the Alloys of Gold, to which I always refer with pleasure, we find some extraordinary instances of anomalies in specific gravity, both in excess and diminution upon the calculated mean. His experiments have not been doubted; nor can their accuracy be called in question. The principle of deviation in the true and the calculated mean is therefore admitted. Who then can say where this deviation shall end, or mark out limits to the operations of nature?

Particularly the different specific gravities of gaseous water or steam and a mixture of the gases which form it. These are as 7 to 4.

But a no less extraordinary instance of irregular density is daily before our eyes; yet it has not so much as attracted our attention. It is true that it is taken from among the gases. But, if we suppose that we have attained accuracy in experiments upon these subjects, I see no reason to refuse their evidence in this instance. The density of oxygen gas, to that of water is as 1 to 740; and the density of hydrogen gas as 1 to 9792. The mean density of that proportion of oxygen and hydrogen gases which constitutes water, is to that of water as 1 to 2098; or, in other words, water is 2098 times heavier than the mean density of its elements in the gaseous state. But water is only 1200 times heavier than steam, or water in the state of vapour. Therefore, there is a variation in $\frac{1}{4}$, of 898, or nearly half, between the density of water and its elements, when both are in the aeriform state. This fact, however, regards bodies only as they remain in the same state, whether of solidity, liquidity, or fluidity. The anomaly is much greater, if we contemplate them as they pass from one of these states to the other. Yet we must not omit the consideration of such a change, in the instance of mercury alloyed with platina; for the former metal, before liquid, becomes solid as it enters into the new combination.

The fixation of mercury is a fact analogous to many others, and ought to excite no prejudice.

A stronger prejudice will perhaps exist against the fixation of so volatile a substance as mercury. It is certain that the labours of the alchemists have thrown some ridicule upon this subject, as a philosophic pursuit. Men of science have long since declined the research; and it is not probable that we are indebted to experiments undertaken in the true spirit of philosophy, for the present fixation of mercury. But, the same cause which induced us to look upon the project as chimerical, should

should dispose us to admit it when accomplished. Every chemist well knows that similar fixations of volatile substances are not uncommon. If an ore containing sulphur, or arsenic, or antimony, be gently roasted, a great part of those volatile bodies is driven off; but, if a fusing heat be suddenly applied, the mass unites in such a manner that a very small share of them escapes. Mr. Hatchett has instanced an artificial combination of gold and arsenic, from which he could not expel the latter metal, by any degree of heat. Yet arsenic, though less fusible, is not much less volatile than mercury. I will also add a case still more in point; viz. the combination of arsenic and platina, which is not to be broken by a fusing heat.

Instances; the volatile bodies sulphur, arsenic, antimony.

An example of this fact, occurs again in water. The liquefaction or solidification of two gases to produce water, by a loss of caloric, never shocks our mind, because it is familiar to us. We cannot say what loss of caloric may be sustained by mercury, in order to unite with platina; or how far the presence of the latter may contribute to expel caloric from the former. We know too, that at any temperature, without the aid of a combustible body, to act as a reductive, we have not been able to disunite the last portions of oxygen, from the oxides of iron or of manganese. Yet, in the usual method of reducing a metallic oxide, the oxygen is surrounded by a much greater quantity of caloric than is necessary to convert it into gas. Every fixation of a volatile substance is analogous to the present question; and they whose minds have taken alarm from the novelty of the fact, may thus be familiarized with the necessity of admitting it.

Mercury having lost caloric and combined with platina, may be in the same situation as oxygen in many oxides; viz. not expellable by mere heat, and perhaps by few affinities.

But, it may be objected, in the instances of iron or manganese, oxygen is combined with a combustible body, and retained in it by a decided and powerful affinity. There is no reason to suppose that such an affinity may not exist among metals. We have been forced to acknowledge it, in a few cases, among the earths; and, from the profound and sagacious researches of Mr. Berthollet, we have learned many new facts, that promise us a rapid increase of knowledge. I shall beg leave to add a few examples, which are taken from that class of bodies to which the subject of the present Paper belongs, and show that the metals obey the general law of mutual attraction.

Metals may combine together by an affinity comparable to that of a metal and oxygen.

(To be concluded in our next.)

III.

*Investigation of certain Theorems relating to the Figure of the Earth. By JOHN PLAYFAIR, F. R. S. Edin. and Professor of Mathematics in the University of Edinburgh. **

No complete results have yet been had as to the figure and magnitude of the earth.

Enumeration.

The results of observed eccentricity agree with the computations deduced from the length of the seconds pendulum.

1. THE observations which have been made to determine the magnitude and figure of the earth, have not hitherto led to results completely satisfactory. They have indeed demonstrated the compression or oblateness of the terrestrial spheroid, but they have left an uncertainty as to the quantity of that compression, extending from about the one hundred and seventieth, to the three hundred and thirtieth part of the radius of the equator. Between these two quantities, the former of which is nearly double of the latter, most of the results are placed, but in such a manner that those best entitled to credit are much nearer to the least extreme than to the greatest. Sir Isaac Newton, as is well known, supposing the earth to be of uniform density, assigned for the compression at the poles $\frac{1}{230}$, nearly a mean between the two limits just mentioned; and it is probable, that, if the compression is less than this, it is owing to the increase of the density towards the centre. Boscovich, taking a mean from all the measures of degrees, so as to make the positive and negative errors equal, found the difference of the axes of the meridian $= \frac{1}{244}$. By comparing the degrees measured by Father Leisganig in Germany, with eight others that have been measured in different latitudes, La Lande finds $\frac{1}{211}$, and, suppressing the degree in Lapland, which appears to err in excess, $\frac{1}{211}$ for the compression. La Place makes it $\frac{1}{211}$; Sejour $\frac{1}{267}$, and, lastly, Carouge and La Lande $\frac{1}{266}$.

These results, which reduce the eccentricity of the meridians so much lower than was once supposed, agree well with the observations of the length of the pendulum made in different latitudes. Were the earth a homogeneous body, Sir Isaac Newton demonstrated, that the diminution of gravity under the equator would be $= \frac{1}{230}$, expressed by the same fraction with the compression at the poles. M. Clairault made afterwards a very important addition to this theorem: for he shewed, that, if the earth be not homogeneous, but have a

* Edinburgh Trans. Vol. V.

density that varies with any function of the distance from the centre, the two fractions, expressing the compression at the poles, and the diminution of gravity at the equator, when added together, must be of the same amount as in the homogeneous spheroid, that is, must be $= \frac{2}{230}$ or $\frac{1}{115}$. Now, the second pendulum is concluded, from the best and most recent observations, to be longer at the pole than at the equator by $\frac{1}{133}$, and this, taken from $\frac{1}{115}$, leaves $\frac{1}{304}$ for the compression at the poles.

2. But though $\frac{1}{300}$, or some fraction not very different from it, should be admitted as the most probable value of the compression, or ellipticity, as it is called, of the terrestrial spheroid, it still remains to be explained, why all the observations, considering the care with which they have been made, do not agree more nearly with this conclusion. Among the causes that may be assigned for this inconsistency, though unavoidable mistakes, and the imperfection of instruments, must come in for a part, there can be little doubt that local irregularities in the direction of gravity have had the greatest share in producing it. Of these irregularities, that which arises from the attraction of mountains has had its existence proved, and its quantity, in one case, ascertained, by the very accurate observations of the present astronomer-royal at Schehallien in Perthshire. We may trace the operation of this cause in many of the degrees that have been actually measured. Thus, in the degree at Turin, when divided into two parts, and each estimated separately, that which was to the north of the city, and pointed toward Monte Rosa, the second of the Alps in elevation, and the first perhaps in magnitude, was found greater in proportion than that toward the south, the plummet having been attracted by the mountain above mentioned, and the zenith made of consequence to recede toward the south. There are no doubt situations in which the measurement of a small arch might, from a similar cause, give the radius of curvature of the meridian infinite, or even negative.

But there is another kind of local irregularity in the direction of gravity, that may also have had a great effect in disturbing the accuracy of the measurement of degrees. The irregularity I mean is one arising from the unequal density of the materials under and not far from the surface of the earth; and this cause of error is formidable, not only because it may go to a great extent,

Why have not the observations of axes agreed better?

Ans.—Principally because the attractions of mountains have caused error in the use of the plumb-line.

Another more formidable cause of error:

that the density of the strata near the earth's surface must be much greater towards some regions than others.

This error may be ten or twelve seconds.

Accurate results are therefore only to be had from large arcs;

and these require new modes of computation.

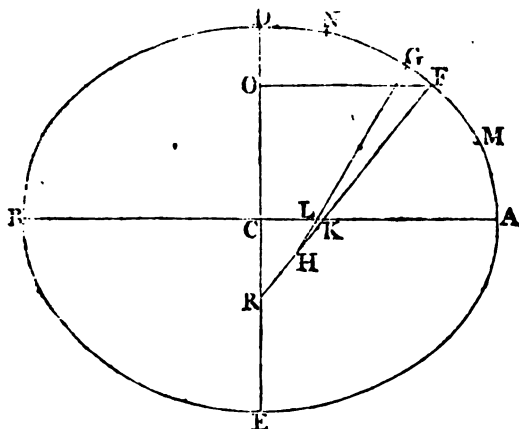
Investigation of formulas for computing the figure of the earth.

extent, but because there is not any visible mark by which its existence can always be distinguished. The difference between the primary and secondary strata is probably one of the chief circumstances on which this inequality depends. The primary strata, especially if we include among them the granite, may often have three times the specific gravity of water, whereas the secondary, such as the marly and argillaceous, frequently have not more than twice the specific gravity of that fluid. Suppose, then, that a degree is measured in a country where the strata are all secondary, and happens to terminate near the junction of these with the primitive or denser strata, the line of which junction we shall suppose to lie nearly east and west; the superior attraction of the denser strata must draw the plummet toward them, and make the zenith retire in the opposite direction; thus diminishing the amplitude of the celestial arch, and increasing, of consequence, the geodetical measure assigned to a degree. From suppositions, no way improbable, concerning the density and extent of such masses of strata, I have found, that the errors, thus produced, may easily amount to ten or twelve seconds.

3. While we continue to draw our conclusions, about the figure of the earth, from the measurement of single degrees, there appears to be no way of avoiding, or even of diminishing, the effects of these errors. But if the arches measured are large, and consist each of several degrees, though there should be the same error in determining their celestial amplitudes, the effect of that error, with respect to the magnitude and figure of the earth, will become inconsiderable, being spread out over a greater interval; and it is, therefore, by the comparison of two such arches that the most accurate result is likely to be obtained. But, in pursuing this method, since the arches measured cannot be treated as small quantities, or mere fluxions of the earth's circumference, the calculation must be made by rules quite different from those that have been hitherto employed. These new rules are deduced from the following analysis.

4. Let the ellipsis *ADBE* represent a meridian passing through the poles *D* and *E*, and cutting the equator in *A* and *B*. Let *C* be the centre of the earth, *AC*, the radius of the equator, = *a*, and *DC*, half the polar axis, = *b*. Let *FG* be any very small arch of the meridian, having its

its centre of curvature in H; join HF, HG cutting AC in K and L. Let ϕ be the measure of the latitude of F, or the measure of the angle AKF, expressed, not in degrees and minutes, but in decimals of the radius 1; then the excess of the angle ALG above AKF, that is, the angle LHK or GHF will be $= \phi$, and therefore $FG = \phi \times FG$. Also, if the elliptic arch $AF = z$, $FG = z = \phi \times FH$.



But FH, or the radius of curvature at F, is =

$$\frac{a^2 b^2}{(a^2 - a^2 \sin^2 \phi + b^2 \sin^2 \phi)^{\frac{3}{2}}} = a^2 b^2 (a^2 - a^2 \sin^2 \phi + b^2 \sin^2 \phi)^{-\frac{3}{2}}$$

$\sin^2 \phi)^{-\frac{3}{2}}$, as is demonstrated in the conic sections. Therefore, if c be the compression at the poles, or the excess of a above b , $b^2 = a^2 - 2ac + c^2$, or because c is small in comparison of a , if we reject its powers higher than the first, $b^2 = a^2 - 2ac$, and $FH = a^3 (a - 2c) (a^2 - a^2 \sin^2 \phi + a^2 \sin^2 \phi - 2ac \sin^2 \phi)^{-\frac{3}{2}} = a^3 (a - 2c) (a^2 - 2ac \sin^2 \phi)^{-\frac{3}{2}}$.

But $(a^2 - 2ac \sin^2 \phi)^{-\frac{3}{2}} = a^{-3} (1 - \frac{2c}{a} \sin^2 \phi)^{-\frac{3}{2}} = a^{-3} (1 + \frac{3c}{a} \sin^2 \phi)$ nearly, rejecting, as before, the terms that involve c^2 , &c. Hence $FH = (a - 2c) (1 + \frac{3c}{a} \sin^2 \phi) = a - 2c + 3c \sin^2 \phi$.

Now

Investigation of
formulas for
computing the
figure of the
earth.

Now $\dot{z} = \dot{\phi} \times FH$, therefore $\dot{z} = \dot{\phi} (a - 2c + 3c \sin^2 \phi)$
 $= (a - 2c) \dot{\phi} + 3c \dot{\phi} \sin^2 \phi$. But $\sin^2 \phi = \frac{1 - \cos 2\phi}{2}$, there-
 fore $\dot{z} = (a - 2c) \dot{\phi} + \frac{3}{2} c \dot{\phi} - \frac{3c \dot{\phi}}{2} \cos 2\phi$, and taking the fluent

$z = \left(a - \frac{c}{2}\right) \phi - \frac{3c}{4} \sin 2\phi$. To this value of z no constant
 quantity is to be added, because it vanishes when $z = 0$.

Therefore an arch of the meridian, extending from the
 equator to any latitude ϕ , is $= a\phi - \frac{c}{2} \left(\phi + \frac{3}{2} \sin 2\phi\right)$.

5. This theorem is also easily applied to measure an arch of
 the meridian, intercepted between any two parallels of the
 equator.

Thus, if MN be any arch of the meridian, ϕ' the latitude of
 M , one of its extremities, and ϕ'' that of N , its other extremity,

we have $AM = a\phi' - \frac{c}{2} \left(\phi' + \frac{3}{2} \sin 2\phi'\right)$, and

$AN = a\phi'' - \frac{c}{2} \left(\phi'' + \frac{3}{2} \sin 2\phi''\right)$, therefore the arch

$MN = a(\phi'' - \phi') - \frac{c}{2} \left((\phi'' - \phi') + \frac{3}{2} \sin 2\phi'' - \frac{3}{2} \sin 2\phi'\right)$.

6. If, therefore, MN be an arch of several degrees of the
 meridian, the length of which is known by actual measure-
 ment, and also the latitude of its two extremities M and N ,
 this last formula gives us an equation, in which a and c are
 the only unknown quantities. In the same manner, by the
 measurement of another arch of the meridian, an equation will
 be found, in which a and c are likewise the only unknown
 quantities. By a comparison, therefore, of these two equa-
 tions, the values of a and c , that is, of the radius of the
 equator, and its excess above half the polar axis, may be de-
 termined.

Thus, if l be the length of an arch measured, m the co-effi-
 cient of a , and n of c , computed by the last formula; and if l'
 be the length of any other arch, m' the coefficient of a , and n' of
 c , computed in the same manner, we have $ma - nc = l$,

and $m'a - n'c = l'$.

Whence $a = \frac{n'l - nl'}{mn' - m'n}$; $c = \frac{m'l - ml'}{mn' - m'n}$ and $\frac{c}{a} = \frac{m'l - ml'}{n'l - nl'}$. It
 may

may be useful, in the numerical calculation, to observe also that

$$c = \frac{na - l}{n}.$$

7. The arch of the meridian, which was measured in Peru, compared with that measured in France, will afford an example of the application of these formulas.

The amplitude of the arch measured in Peru was $3^{\circ} 7' 1''$, and its length 176940 toises. To reduce this to the level of the sea, above which it was elevated 1226 toises, 66 toises must be subtracted, and again 12 toises added to adapt it to the mean temperature of the atmosphere. Thus corrected it is 176886 toises. The arch measured begun $36''$ north of the equator, and extended to the parallel of $3^{\circ} 6' 25''$ south; we shall suppose it to have begun under the equator, and to have extended to the parallel of $3^{\circ} 7' 1''$, a supposition which can produce no sensible error, and will somewhat simplify the calculation. Thus ϕ , in the preceding formula, is an arch of $3^{\circ} 7' 1''$ expressed in decimals of the radius 1, and so we have $m = .0544009$, $n = .1086408$, and $l = 176886$.

Again, the amplitude of the whole arch measured in France from Dunkirk to Perpignan is $8^{\circ} 20' 2''\frac{1}{2}$ and its length 475496 toises. The northern extremity of this arch is in latitude $51^{\circ} 2' 1''$, and the southern in $42^{\circ} 41' 58''\frac{1}{2}$. Hence $\phi' = .8907045$, and $\phi' = .7452459$, and therefore $m' = .1454586$, $n' = .0585735$, $l = 475496$.

Therefore, $a = \frac{n'l - nl'}{nn' - m'n} = 3273325$ toises;

$$c = \frac{m'l - ml'}{nn' - m'n} = 10917 \text{ toises,}$$

$$\text{and } \frac{c}{a} = \frac{1}{300} \text{ nearly.}$$

Wherefore also the longer axis of the meridian is to its conjugate, or a is to b as 300 to 299.

This proportion agrees well with that which was already pointed out as the most probable result, from the comparison of single degrees, and from observations of the pendulum. As these conclusions are obtained by different methods, they tend greatly to confirm one another.

8. From this too it seems highly probable, that the uncertainty which yet remains with respect to the true figure of the earth will be well determined

Application of the formulas to the arc measured in Peru and that in France.

Probability that the figure of the earth will be well determined

by measurement of some other considerable arcs.

earth will be entirely removed by the measurement of some other considerable arches of the meridian. Such an arch will be furnished by the survey of Great Britain begun by General Roy, and still continued in a style of accuracy so much superior to any other system of geometrical operations that has ever yet been executed. In drawing the conclusions from observations made with such exactness, it may be necessary to employ a more accurate approximation than has been done in the preceding formula, by retaining the second power of c . The equations to be resolved will thus become of the second order, but as the unknown quantities can be nearly found by the solution of a simple equation, the farther approximation to their true values will be accompanied with no difficulty.

Farther approximation to be then employed.

9. Concerning this farther approximation it may be useful however to remark, that if c^2 be retained, its coefficient in the

formula of § 4 will be $\frac{1}{16a} \left(\phi + \frac{15}{4} \sin 4\phi \right)$;

and therefore in the formula of § 5 it will be

$$\frac{1}{16a} \left(\phi'' - \phi' + \frac{15}{4} (\sin 4\phi'' - \sin 4\phi') \right).$$

If then the quantity

$$\frac{1}{16a} \left(\phi'' - \phi' + \frac{15}{4} (\sin 4\phi'' - \sin 4\phi') \right),$$

computed for any arch of the meridian, be put $= g$, and the same, computed for any other arch, be $= g'$, the equations of § 6 will become,

$$ma - nc + \frac{gc^2}{a} = l, \text{ and}$$

$$m'a - n'c + \frac{g'c^2}{a} = l'.$$

10. Here if we put d for the value of a , as given by the for-

mula $\frac{n'l - n'l'}{mn' - m'n}$; and h for the value of c , as given by the for-

mula $\frac{m'l - ml'}{mn' - m'n}$, also v for the correction to be made on d , and

for the correction to be made on h , so that $a = d + v$, and

$c = h + u$, by substituting these values of a and c in the two

last equations we have $mv - nu + \frac{g(h+u)^2}{d+v} = 0$, and

$$m'v - n'u + \frac{g'(h+u)^2}{d+v} = 0.$$

Hence

Hence, rejecting all the terms that involve v^2 , u^2 , or uv , we have $dmv - dnu + gh^2 + 2ghv = 0$, and $dm'v - dn'u + g'h^2 + 2g'hv = 0$. Farther approximation to be then employed.

Therefore, $v = \frac{(ng' - n'g) h^2}{(n'm - nm') d + (gn' - g'n) 2h}$, also

$$u = \frac{g'h^2 (dm + 2gh) - gh^2 (dn' + 2g'h)}{dn' (dm + 2gh) - dn (dm' + 2g'h)}$$

And again, by rejecting those terms that are small in comparison of the rest, $v = \frac{h^2 (ng' - n'g)}{d (n'm - nm')}$, and

$$u = \frac{h^2 (g'm - gm)}{d (n'm - nm')}.$$

Thus v and u are found, and of consequence $d+v$ and $h+u$, that is a and c , without neglecting any terms that are not of an order less than $\frac{c^2}{a}$; and when it is considered that

$$\frac{c^2}{a}$$

$\frac{c^2}{a}$ is less than $\frac{1}{22500}$, it will readily be allowed that it is quite

unnecessary to carry the approximation farther.

11. The same thing that renders the comparison of large arches of the meridian useful for lessening the effect of errors arising from irregularities in the direction of gravity, makes it serve to diminish the effect of all the errors of the astronomical observations at the extremities of the arches, from whatever cause they arise. They are all diffused over a greater interval, and have an effect proportionally less in diminishing the accuracy of the last conclusion. The errors of observation are less the larger the arcs.

12. The measurement therefore of large arches of the meridian, especially if performed in distant countries, is likely to furnish the best data for ascertaining the true figure of the earth; and on this account extensive and accurate surveys, such as that above mentioned, are no less interesting to science, in general, than conducive to national utility. The survey of this island, when completed, will furnish an arch of the meridian, beginning at the same parallel where that measured in France terminates, and nearly of the same extent, so that the length of an arch of more than 16° , or almost a twentieth of the earth's circumference, will become known. The different portions of this arch compared with one another, or with the Extensive surveys particularly useful.
arch

arch measured in Peru, will afford a variety of *data* for determining the true figure of the earth.

Observations of perpendiculars to the meridian, and of parallels of latitude.

But surveys of the kind now referred to, afford likewise other materials from which the solution of this great geographical problem may be deduced. These are chiefly of two sorts, viz. the magnitude of arches, either of the curves perpendicular to the meridian, or of the circles parallel to the equator. Examples of the first of these have been given by General Roy and Mr. Dalby; the observations which follow are directed toward both.

Large perpendiculars are of very difficult measurement;

and small ones erroneous from partial gravitation.

13. With respect to the measurement of arches perpendicular to the meridian, it may be observed, that the directions of gravity at different points of such arches do not intersect one another at all, unless the distances of those points from the said meridian be very small. On this account the measurement of a large arch perpendicular to the meridian would involve in itself considerable difficulty; to avoid which it is necessary that the arch measured be but small, or one that does not greatly exceed a single degree. Such measurements are of course obnoxious to all the errors that arise from the deflection of the plumb-line, and cannot therefore furnish *data* for determining the figure of the earth, equally valuable with those which may be derived from large arches of the meridian. The method of determining the figure of the earth, from degrees of the perpendicular to the meridian, is not however without its advantages, and in certain circumstances is preferable to any other that proceeds by the measurement of arches equally small. This method is twofold; as a degree of the meridian may be compared with a degree of the perpendicular to it in the same latitude; or two degrees perpendicular to the meridian, in different latitudes, may be compared with one another. The advantages peculiar to each will appear from the following investigation.

To find the axes of a spheroid from comparing a deg. of the merid. with one of the perpend.

14. Let it be required to find the axes of an elliptical spheroid, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the same latitude.

Let the ellipsis *ADBE* (Fig. on page 105) represent a meridian, of which a degree is measured at *F*. Let the perpendicular

itude of F, or the angle OFR = ϕ . Also let AC = a ,
 = b , and $a - b = c$, as before.

Then from the nature of the ellipsis, FO =

$$\frac{a^2 \cos \phi}{\cos^2 \phi + b^2 \sin^2 \phi}, \text{ and because } \sin FRO : 1 :: FO : FR,$$

$$\text{so, } \cos \phi : 1 :: FO : FR, FR = \frac{a^2}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}};$$

this, therefore, is the radius of curvature of the section of
 the spheroid perpendicular to the meridian at F. But the ra-
 dius of curvature of the meridian at F, that is FH =

$$\frac{a^2 b^2}{\cos^2 \phi + b^2 \sin^2 \phi}, \text{ therefore}$$

$$FH :: \frac{a^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{1}{2}}} : \frac{a^2 b^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{3}{2}}},$$

dividing both by $\frac{a^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{1}{2}}}$, we have

$$FH :: a^2 \cos^2 \phi + b^2 \sin^2 \phi : b^2,$$

If then D be the length of a degree of the meridian at F
 and D' the length of a degree of the circle at right angles to it,

$$D :: a^2 \cos^2 \phi + b^2 \sin^2 \phi : b^2, \text{ and } \frac{D'}{D} =$$

$$\frac{\cos^2 \phi + b^2 \sin^2 \phi}{b^2} = \frac{a^2}{b^2} \cos^2 \phi + \sin^2 \phi. \text{ Hence}$$

$$\sin^2 \phi = \frac{a^2}{b^2} \cos^2 \phi \text{ and } \frac{a}{b} = \frac{\sqrt{\frac{D'}{D} - \sin^2 \phi}}{\cos \phi}.$$

This last formula, therefore, gives the ratio of a to b when

To find the axes
of a spheroid
from comparing
a deg. of the
merid. with one
of the perpend.

16. To find a and b themselves, if $m = 57.2957$, &c. or the number of degrees in the radius, so that $mD' = FR =$

$$\frac{a^2}{(a^2 \cos^2 \phi^2 + b^2 \sin^2 \phi^2)^{\frac{1}{2}}}, \text{ and since it has been already}$$

$$\text{shewn that } \frac{a^2}{b^2} = \frac{\frac{D'}{D} - \sin^2 \phi^2}{\cos^2 \phi^2}, \text{ or } b^2 = \frac{a^2 \cos^2 \phi^2}{\frac{D'}{D} - \sin^2 \phi^2}, \text{ there-}$$

$$\text{fore } mD' = \frac{a^2}{\left(a^2 \cos^2 \phi^2 + \frac{a^2 \cos^2 \phi^2}{\frac{D'}{D} - \sin^2 \phi^2} \times \sin^2 \phi^2 \right)^{\frac{1}{2}}} =$$

$$\frac{a}{\cos \phi \left(1 + \frac{\sin^2 \phi^2}{\frac{D'}{D} - \sin^2 \phi^2} \right)^{\frac{1}{2}}} \text{ and } a =$$

$$mD' \cos \phi \sqrt{1 + \frac{\sin^2 \phi^2}{\frac{D'}{D} - \sin^2 \phi^2}}.$$

$$\text{Now, } 1 + \frac{\sin^2 \phi^2}{\frac{D'}{D} - \sin^2 \phi^2} = \frac{\frac{D'}{D}}{\frac{D'}{D} - \sin^2 \phi^2} = \frac{1}{1 - \frac{D}{D'} \sin^2 \phi^2},$$

$$\text{therefore } a = \frac{mD' \cos \phi}{\sqrt{1 - \frac{D}{D'} \sin^2 \phi^2}}.$$

17. This value of a is very convenient for logarithmical calculation; for if $\sin \phi \sqrt{\frac{D}{D'}}$ be computed, it will always be less than 1, because D' is greater than D , and therefore may be taken for the sine of an arch ψ , of which arch $\sqrt{1 - \frac{D}{D'} \sin^2 \phi^2}$ will of course be the cosine, so that $a = \frac{mD' \cos \phi}{\cos \psi}$.

The same method may be used for finding $\frac{a}{b}$ from the formula in § 15.

In

In the same manner that a has been found, we will obtain

$$b = \frac{mD' \cos \varphi^2}{\left(1 - \frac{D}{D'} \sin \varphi^2\right) \sqrt{\frac{D'}{D}}}$$

To find the axes of a spheroid from comparing a deg. of the merid. with one of the perpend.

If we examine these formulas in the extreme cases, viz. when $\varphi = 90^\circ$, and when $\varphi = 0$, we shall have in the for-

mer case $a = \frac{0}{0}$, because $\cos \varphi = 0$, and also $D' = D$, so

that $1 - \frac{D}{D'} \sin^2 \varphi = 0$. Here therefore a is indefinite, and

may be of any magnitude whatever; and it is evident that this is the result which the formula ought to give: because at the pole, or when $\varphi = 90^\circ$, the perpendicular to the meridian is itself a meridian, and therefore the measurement of the two degrees, D and D' , is but the same with the measurement of one degree.

When $\varphi = 0$, that is at the equator, the circle perpendicular to the meridian is the equator itself, and we have then $a = mD'$, a being determined in this case by the degree of the equator alone. Here also we have $\frac{a}{b} = \sqrt{\frac{D'}{D}}$, which is known to be true.

15. The preceding formulas may be rendered more simple, if we aim only at an approximation, which indeed is all that is necessary in this inquiry. Since c denotes the compression, or since $a - c = b$, and therefore $a^2 - 2ac = b^2$ nearly, consequently the radius of curvature of the meridian at F , that is

$$mD = \frac{a^2 (a^2 - 2ac)}{(a^2 - 2ac \sin \varphi^2)^{\frac{3}{2}}} = \frac{a^3 (a - 2c)}{a^2 \left(1 - \frac{2c}{a} \sin \varphi^2\right)^{\frac{3}{2}}} =$$

$$(a - 2c) \left(1 - \frac{3c}{a} \sin \varphi^2\right), \text{ or } mD = a - 2c + 3c \sin \varphi^2. \text{ In}$$

the same manner $mD' = a + c \sin \varphi^2$. From these equations we obtain, rejecting always the higher powers of c ,

$$c = \frac{m(D' - D)}{2 \cos \varphi^2}, \quad a = mD' - \frac{m(D' - D) \sin \varphi^2}{2 \cos \varphi^2}; \text{ and}$$

$$\frac{c}{a} = \frac{D' - D}{2D' \cos \varphi^2}.$$

These formulas may be transformed into others a little more convenient for computation, by putting $\sec \varphi^2$ instead of

$\frac{1}{\cos \phi^2}$, and $\tan \phi^2$ instead of $\frac{\sin \phi^2}{\cos \phi^2}$; we have then,

$$c = \frac{m}{2} (D' - D) \sec \phi^2,$$

$$a = mD' - \frac{m}{2} (D' - D) \tan \phi^2, \text{ and}$$

$$\frac{c}{a} = \frac{(D' - D)}{2D'} \sec \phi^2.$$

Example of the compression deduced from the meridian and its perpendicular.

19. We may apply these formulas to the computation of $\frac{c}{a}$, &c. from the degrees of the meridian and perpendicular, measured in the south of England. We find, in one example, (*Phil. Trans.* 1795, p. 537), that $D = 60851$ fathoms, $D' = 61182$, the latitude, or ϕ being $= 50^\circ 41'$. From this $\frac{c}{a} =$

$$\frac{D' - D}{2D' \cos^2 \phi} = \frac{331}{2 \times 61182 \times (\cos 50^\circ 41')^2} = \frac{1}{148.4}, \text{ which}$$

is nearly the same result with that deduced in the passage just referred to. Indeed the solution of this problem, contained in the *Trigonometrical Survey*, is quite unexceptionable; and the theorems here offered are not given as containing a more accurate solution, but one that is in some respects more simple.

Whether the magnitude of this result may prove an irregular figure.

The above compression, if the remarks already made be well founded, is much too great, being more than double of what was obtained from comparing the whole arch of the meridian measured in France with the whole of that measured in Peru. At the same time it is right to observe, that all the other comparisons of the degrees of the meridian, with those of the curve perpendicular to it, made from the observations in the south of England, agree nearly in giving the same oblateness to the terrestrial spheroid. For this circumstance, it is certainly not easy to account; the unparalleled accuracy with which the whole of the measurement has been conducted, makes it in the highest degree improbable that it arises from any error; and even if errors were to be admitted, it is not likely that they should all fall on the same side. The authors of the *Trigonometrical Survey* seem willing, therefore, to give up the elliptic figure of the earth, (*Ibid.* p. 527); but before we abandon that very natural and simple hypothesis, it may perhaps be worth while to attend to the following considerations.

20. In the part of England, where the measures we are now treating of have been taken, the strata are of chalk, and though of great extent, are bordered, on all the sides that we have access to examine, by strata much denser and more compact. Toward the west the chalk is succeeded by limestone, and that limestone by the primitive schistus and granite of the west of Devonshire and of Cornwall. On the east we may suppose that something of the same kind takes place, though the sea prevents us from observing it, as the chalky and argillaceous beds extend in this direction to the coast, and probably to some distance beyond it. Now the meridian of Greenwich may be considered as dividing the tract of country, occupied by these lighter strata, into two parts, in such a manner, that the plummet being carried to a distance from it, either east or west, approaches to the denser strata, and is of course attracted by them, so that the zenith is forced back, as it were, to the meridian of Greenwich, and does not recede from it, in the heavens, at so great a rate as the plummet itself does, on the earth. Hence the longitudes from this meridian, estimated by the arches in the heavens, intercepted between the zenith and the said meridian, will appear less than they ought to do; and too much space on the surface of the earth will of consequence be assigned as the measure of a degree. In this way D' is made too great; and we may suppose the circumstances such that D , on going north or south, is not enlarged in the

Observations to show that the irregularity probably arises from local circumstances.

The meridian of Greenwich appears to divide a chalk country, bearing on each side upon denser strata of limestone, schistus, and granite, which, by attracting the plumb-line either way outwards, must require a longer measure of easting or westing to correspond with an apparent celestial arc.

same proportion; hence $\frac{D' - D}{D'}$ will be augmented, and of

course $\frac{c}{a}$ will be represented as too great. This explanation

may perhaps appear very hypothetical, and it is certainly proposed merely as a hypothesis. It is a hypothesis, too, that lays claim only to a temporary indulgence, as it is proposed at the very moment when it may be brought to the trial, and when, by a further continuation of the survey toward the north, it will probably be determined how far the distribution of the strata of this country affects the direction of gravity. It will indeed be curious to remark what irregularities take place on advancing into the denser strata of the north. The limestone and sandstone strata of the middle part of the island will succeed to the chalk of the south, the primitive and denser strata still

occupying the west, at least at intervals, as in Wales, Cumberland, and Galloway. Further to the north, that is, beyond the Tay, the strata became entirely primitive, most of them of the densest kind, and in the interior of the island, with a very few exceptions, continue the same to its most northern extremity. In the survey of Britain, therefore, several situations must occur where the plummet, passing from lighter to denser strata, ought to give indications of some irregularities in the direction of the gravitating force. It will be seen hereafter how far these conjectures are verified by experience.

(To be continued.)

IV.

On the Determination of the Length of the Solar Year. By Mr.
R. WINTER.

*Red Cross Wharf, London-Bridge,
13th Jan. 1804.*

To Mr. NICHOLSON,

SIR,

Determination
of the length of
the solar and
sidereal years.

SHOULD you consider the following communication as deserving a place in your valuable Journal, by inserting it you will oblige

Your's, &c.

R. WINTER.

To determine the precise length of the Solar Year is an object of primary utility in astronomy, as being the basis for ascertaining the periods of all the moving bodies in the universe.

Various and accurate methods have been given for determining this period; but as the following unites simplicity, to the advanced state of science, these characters may render it worthy of your consideration.

Given, the time and place of the sun in the ecliptic, and the time of its returning to the same place; the obliquity of the ecliptic for the present time, together with the decrease thereof, per year, to find the length of the solar year.

According to Ptolemy, the place of the sun was Sagittarius $24^{\circ} 22' 50''$ anno. 130, Dec. 13th, $11^{\text{h}} 38^{\text{m}} 40^{\text{s}}$ (reduced to the

the meridian of London,) its return to that place in 1804 according to Mayers tables improved, is Dec. 13th. $0^h 19^m 36^s$ the interval, (allowing for bissextiles, &c.) is $611416^d 12^h 40^m 56^s$ which divided by 1674, the number of years, gives $365^d 5^h 49^m 42^s$. Now the obliquity of the ecliptic, as observed by Cassini in 1655, and Flamsteed in 1689, being compared with that observed by Hornsby in 1772, Maskelyne in 1769, Bradley in 1750, and Mayer in 1756, and a mean of their observations taken, will give for the decrease of the obliquity of the ecliptic $59''$ per century, or $35''.4$ per year. According to these calculations the obliquity of the ecliptic for 1804 will be about $23^\circ 27' 49''$; then as the sun moves over this space in one quarter of a year, say as $23^\circ 27' 49''$ is to $\frac{1}{4}$ of the given year, so is the decrease of the obliquity of the ecliptic per year $35''.4$ unto $55''$, which taken from $365^d 5^h 49^m 42^s$ gives for the length of the solar year $365^d 5^h 48^m 47^s$ and the time the sun moves over the procession of the equinoxes is 20 minutes 27 seconds, which being added to the solar year will give $365^d 6^h 9^m 14^s$ for the length of the sidereal year.

But by assuming $\frac{1}{4}$ of the year thus found, and working as before, it will give the length of the year more accurately.

V.

Extracts from a Letter of R. Chenevix, Esq. F.R.S. from Dresden, to Charles Hatchett, Esq. F.R.S. containing Information respecting the new Metal contained in Crude Platina; Beet Sugar; the non-existence of Agustile as a peculiar Earth; and a Description of a new Furnace for Chemical Operations. Communicated by C. HATCHETT, Esq.

I SUPPOSE you have heard of the new metal contained in raw platina. Here is what Berthollet has just written to me upon the subject:

“Des Cossils dissolves raw platina in nitro-muriatic acid, and precipitates it by muriate of ammonia at several times; the first portions are yellow, the last redder. He reduces the red precipitate, and obtains an alloy. He exposes this alloy to a current of oxygen, and a blue oxide is volatilized; pure platina remains behind. The blue sublimate is the oxide of his
new

New metal in
crude platina.

new metal; on the same day Fourcroy and Vauquelin read their paper at the Institute, and mentioned similar experiments."

Palladium asserted to be an alloy of the new metal with mercury.

Van Mons writes me word that at Paris they say that it is not platina, but this new metal, which with mercury composes palladium; but that has very little to say to the purpose, for the singular thing in palladium is, not what regards platina, but what regards mercury, and the fusibility of the combination. Besides there must have been platina in my palladium, as I took the metal reduced from the red and yellow salts indiscriminately.

Manufacture of beet sugar by Lampadius.

Lampadius tells me that he has made several hundred weight of sugar from the red beet; but the manufactory he established has failed. It cost him within two gros per lb. as much as the common sugar of equal quality, but then he had a residuum fit for fermentation, which was clear profit.

Agustite is merely phosphate of lime.

I had made some progress in the analysis of the Saxon beryl, when I received a letter from Berthollet, in which he told me that Vauquelin had found the agustite to be nothing more than phosphate of lime. I continued my experiments however, and obtained the following results. Having boiled muriatic acid upon the pulverized mass which contains the Saxon beryl, I obtained a solution of the pretended agustite. I precipitated by ammonia, and, upon examining the precipitate, found it to be phosphate of lime. To have no doubt as to the phosphoric acid, I treated 1500 grains of this precipitate with half its weight of sulphuric acid, washed the mass, and evaporated to dryness. I re-dissolved as much as was soluble in distilled water, and saturated the liquor by ammonia. By evaporating once more, I obtained phosphate of ammonia, which I decomposed at a strong red heat, and obtained phosphoric acid, which gave no trace of sulphuric or any other acid, or of any earth, alkali, or metal. I mixed this with pounded charcoal, and by distillation in a strong fire obtained a considerable quantity of phosphorus. Thus then the simple earth of Mons, Tromsdorf is phosphate of lime.

Muriatic solution of Saxon beryl precip. by amm. gave phosph. of lime. This was decomposed by sulph. acid; and the purified phosphoric acid reduced to actual phosphorus.

I have

I have constructed a wind furnace here, which is in some respects to be preferred to the usual form. The sides, instead of being perpendicular, are inverted, so that the hollow space is pyramidal. At the bottom the opening is thirteen inches square,



and at the top but eight. The perpendicular height is 17 inches. This form appears to me to unite the following advantages. 1st. A great surface is exposed to the air; which, having an easy entrance, rushes through the fuel with great rapidity. 2d. The inclined sides act in some measure as reverberating surfaces: And 3d. The fuel falls of itself, and is always in close contact with the crucible, placed near

Chemical furnace. Frustum of a cone, narrowest above. Its great advantage is that the fuel descends without the help of stirring.

the grate. This I believe to be the principal advantage. The late Dr. Kennedy of Edinburgh, whose opinion on this subject claims the greatest weight, found that the strongest heat in our common wind furnaces was within two or three inches of the grate. That therefore is the most advantageous position for the crucible, and still more so when we can keep it surrounded with fuel. It is inconvenient and dangerous for the crucible to stir the fire often to make the fuel fall, and the pyramidal form renders this unnecessary. It is also more easy to avoid a sudden bend in the chimney by the upper part of the furnace advancing as in this construction. *a* is a grate; *c* and *c* are two bricks, which I can let in at pleasure to diminish the capacity; *b* is another grate, which I can place upon the bricks *c* and *c* for smaller purposes; *d* and *d* are bricks which I can place upon the grate *b* to diminish the upper capacity, so that in fact I have four different sizes in the same furnace. I have had some very strong fires in using the whole capacity from the grate *a* to the top *e* without the bricks *c* and *c*; but I am at a loss for our good English coaks. The bricks have all been ground down to the slope of the furnace and fit in with tolerable accuracy. They are totally independent of the pyramidal form, &c. of the furnace.

VI.

*Account of an Experiment to imitate the Damascus Sword Blades,
In a Letter from Mr. JAMES STODART.*

To Mr. NICHOLSON,

DEAR SIR,

On sword blades
in general.
Quality too
often defective.

Experiment.
Steel and iron
bars were welded
together.

Forged out.

Twisted, flat-
tened, again
welded and ham-
mered flat.

An edge of steel
was welded into
a back of the
compound.

HAVING lately had an opportunity of examining some sword blades, which appeared to be defective, I was induced to make the following experiment. The subject is surely of some importance, and perhaps never more so than at the present moment. We hear of swords having broken in battle, and we can hardly imagine a more distressing circumstance. Those which I have seen are certainly in no danger of failing in that way, for on the contrary they are evidently too soft, and consequently cannot form a good cutting edge. I am not acquainted with the process used in making sword blades, but am inclined to suspect that the price allowed is not equal to the labour necessary to form a good instrument. The following method, which I believe to be nearly the same as that practised at Damascus, but which I suspect would be too difficult and expensive for general application, may perhaps lead to some more simple method of accomplishing the desired purpose. I took six small bars of good malleable iron, and the same number of sheer steel, and laid them one on another alternately, as if forming a galvanic pile; I then with the assistance of an expert workman, committed them to a clean forge fire, and with care we succeeded in welding them into a solid lump. This was forged into a stout flat plate, which being heated to whiteness, was by means of strong tongs twisted spirally until it formed a cylindrical tube. In this twisted state it was heated, hammered flat, and again welded, and after being forged into a convenient form and substance, was doubled throughout its whole length, somewhat in the manner of the back of a saw. A slip of good steel was inserted, and another welding heat taken, which consolidated the whole mass. I need not say this slip of steel was intended for our edge. The remaining part of the process was simple; it consisted only in forging it into the shape of the blade we wanted; which on examination proved perfectly found in every part. Being eager to witness some proofs of excellence and beauty which my expectation had

had anticipated, I too hastily and without due consideration proceeded to harden it by heating and quenching in water; and had the misfortune to see it cracking in seven or eight different places. I have no doubt this was occasioned by the unequal expansion and subsequent contraction of the different parts of the mass. In my next trial I shall guard against this accident. Enough however remained sound to prove it both good and beautiful; the edge bears the severest trials at the same time that the whole blade has sufficient tenacity. I have polished a part of it, and by applying a weak acid, produced an appearance, which though by no means equal to the beauty of what is called the Damascus water, leaves me little reason to doubt of accomplishing that appearance in my next trial. My intention is to multiply my pieces of metal, to repeat the process of twisting, and certainly not again to quench in water. I shall take the liberty to transmit to you an exact account of my next experiment, and if successful, to accompany it with a sample of the metal formed into a blade of some kind or another. I am with much respect,

DEAR SIR,

Your obedient servant,

J. STODART.

Strand, Jan. 19th, 1804.

P. S. Why is the appearance produced on Damascus steel by the application of an acid called the water? Is it not different degrees of oxidation? * and what is the acid best fitted to produce this appearance. I had a paper given me some ten years ago on this subject, by a gentleman whose name I do not know. Unfortunately I have mislaid it.

In addition to what you have published on the subject in your valuable Journal, pray furnish us with any other facts that may have come to your knowledge since that period. The subject appears to me to be worthy of philosophical research, and perhaps of national encouragement.

* I have always supposed steel to be less readily soluble than pure iron; and that the carbon which is seen on the face of the former during the process of damasking, defends it from the acid, while the fibres of iron are etched by corrosion so as to exhibit the peculiar waving lines of this operation. N.

Letter

VII.

Letter from a Correspondent on the Effects of Thunder on fermenting Liquids; the Chemical Action of Sound, and Tremulous Motions, with other Observations.

Newcastle, Dec. 16, 1803.

To Mr. NICHOLSON.

SIR,

CONVERSATION is certainly one of the most desirable means for the increase and dissemination of knowledge, whatever description it may be of, and if the following remarks, arising from an accidental conversation with a scientific friend, be not altogether unworthy of your notice, you may make what use of them you think proper; and if thought worthy of being presented to the public, you are at liberty to make any necessary alteration in the stile that will render them more worthy of the honour I solicit,

Changes produced in beer and in cream by thunder.

Supposed preventative remedy.

The effect supposed to arise from tremulous action.

It has been observed, so often indeed, that it has almost become a popular remark, that the noise of thunder produces a surprising change on beer, and on cream: the first becoming sour, and the last rancid, when they are agitated by this extraordinary sound. It has also been observed, I believe, that the effects of thunder on malt liquors may be prevented by laying a cord over the cask, at each end of which, a stone, or other hard and ponderous substance is suspended and kept in contact with the outside of the vessel containing the liquid liable to be affected by this kind of noise.

Now we may be able to form some judgment of the manner in which these liquids are affected, by attending to the tremulous motion produced on those glasses called singing and musical glasses, when the surface of the first is agitated by the breath; and the liquid in the last by the vibration produced by the finger drawn along the rim of the glass, which varies in its tone in proportion to the quantity of liquid it contains. In the course of the tone or sound, if the glass be pressed with the finger, the sound either ceases or is considerably diminished, and upon application of the finger at the commencement of the sound it will be evidently felt that both the glass and the liquid are in a state of agitation, and that this agitation ceases or stops when the agitated body is touched by another in a quiescent state.

This

This tremor, so communicated, may in my opinion be the commencement of that insensible or rather imperceptible agitation necessary to the production of the different states of fermentation, which when once began, may be continued, provided the circumstances of temperature, &c. be equally favourable as at first. Now we perceive that this motion is produced by sound; * and a sonorous body loses this vibration on being touched, and the sound itself of course ceases. In like manner the intestine motion arising from the vibration occasioned by the noise of thunder, being prevented from taking place by the contact of the suspended stones with the sides of the cask, the ensuing fermentation cannot commence, and the beer is preserved from running into the acetous fermentation.

Which may produce the commencement of the fermentative agitation.

This insensible motion is also necessary to assist the absorption of oxygen, without which neither beer can grow sour, nor can cream become rancid; for violent agitation will not produce the like effect even though accompanied or assisted with the necessary increase of temperature.

It was also observed by the gentleman with whom I was conversing on this subject, that butter by being over much washed or wrought in water loses considerably of its yellow colour, and acquires in a great measure the paleness and consistence of tallow. It might be worth while to make some experiments in this way, how far animal oils or fat are capable of decomposing atmospheric air or water. The circumstances necessary to determine the mutual change on each, are agitation, and a frequent change of surface. The absorption of oxygen would take place in both cases, the residuum of atmospheric air being azotic gas, and that of water hydrogen gas. I had found by experiments, and that some time before I saw the circumstance publicly noticed, I may say, even before the new theory of chemistry was promulgated, that the union between mercury and the fat with which it was combined in the composition of the ointment, became much more intimate and perfect on being kept for some time, and that the rancidity, which I thought arose from the developement of some peculiar acid, was prevented from becoming sensible by that acid being absorbed by the metal. From this circumstance, and from this supposition, I was led contrary to the directions and commands

Decomposition of water or air by oils.

Rancid oils combine better with mercury than fresh.

* I mean among other causes producing it,

of the Royal College of Physicians, to use a little rancid fat or oil, in order to facilitate my labour and favour my indolence, and I was agreeably surpris'd at my success. The College order the purest and sweetest fat to be used; nevertheless the utmost diligence and care in rubbing the mercury with it does not produce such an accurate combination at first as is accomplished, after being set by for a week or two, and this may be made evident by comparing two specimens of ointment made with equal care and diligence, the one being quite recently finished, and the other having afterwards been kept undisturbed for two or three weeks.—I merely offer these hints for the consideration of wiser heads than mine, and should they with this view be honoured with a place in your valuable repository, your indulgence will be most respectfully acknowledged by

Sir,

Yours, &c.

NORTHUMBRIENSIS.

VIII.

Improvement of the Electrical Machine, chiefly with respect to the Rubbers; by Mr. WOLFF, of Hanover.*

Description of
the machine.

The plate.

MY electrical apparatus is a plate machine, constructed on the plan of that described by Van-Marum †, with a few slight alterations, which are principally in the rubbers. The plate is eighteen inches in diameter, and the piece of wood, which receives the screw that fastens the plate to the axis, is an inch and half in diameter. The rubbers (at the extremities of which, toward the axis, are sticks of black sealing-wax, rounded at each end, and overhanging or projecting on three sides) are in contact with the glass for the length of $5\frac{1}{2}$ inches. Consequently the distance from the circular or central piece of wood to the rubbers is $2\frac{1}{2}$ inches, to which extent the plate

* From Gilbert's *Annalen der Physik*, 1802, No. 13. p. 601.

† *Observations sur la Physique*, Vol. 38. p. 437. or *Philosophical Journal*, 4to. Series, No. 2.

is coated on both sides with a sufficient thickness of varnish. The bow of the conductor, which consists of a ball six inches in diameter, to which is added a moveable tube with several fitting pieces that terminate in balls of different sizes, had at first, at each of its extremities, as in Van-Marum's machine, a brass cylinder three inches long and one inch thick, with an hemisphere at each end. But as conductors of this construction are liable to discharge themselves, on account of the small diameter of their hemispheres, even before they have received their full charge, I have preferred putting one brass ball three inches in diameter on the upper end of the superior conductor; and another on the lower end of the inferior conductor. These balls likewise serve the purpose of collecting the fluid excited at the extreme parts; for the balls, both above and below, project considerably beyond the thickness of the plate, and prevent the escape of the fluid as much as is possible, particularly in a machine less high in proportion than that of Van-Marum. The conductor is only $4\frac{1}{2}$ inches distant from the table, consequently only half the distance of Van-Marum's; yet the fluid does not escape, till the conductor is overcharged. The escape of the fluid is prevented likewise by an insulating square of glass 42 inches, made rough and coated with copal varnish, which is placed on the table beneath the lower conductor, its three short feet being let into the table. This square of glass may be used on several occasions as an insulating stand.

The conductor described.

The escape of electricity to a glass plate placed on the table beneath the conductor.

The four rubbers are made of dry walnut wood soaked in amber varnish, and are $5\frac{1}{2}$ inches long, $1\frac{1}{2}$ broad, and a little more than $\frac{1}{4}$ of an inch thick. The metallic plate that communicates with the leather covered with amalgam, is only an $1\frac{1}{2}$ inch broad, and is fixed externally to the centre of the piece of wood. The rubbers are pressed toward the glass by means of a spring. They are covered with a piece of thick woollen, upon which is a piece of fine neat's leather. After the leather is fastened to the wood, it is wetted, and pressed between

The rubbers described.

* This varnish is composed of copal, two ounces; gum sandarac, one ounce; white amber, one ounce; olibanum, half an ounce; powdered and dissolved in one pound of alcohol by digestion in a glass matras. When the solution is perfectly cold, it is passed through the filter.

Amber varnish for coating the plate.

two boards, where it is kept till it is again dry. Thus it is rendered very flat, and its edge very sharp, and all its parts will apply to the surface of the glass. This piece of leather is covered with another a little broader, the rough surface of which is towards the glass, and its lower edge on the side towards which the plate moves; and its upper edge on the other side from which the plate moves, being likewise very sharp. The piece of silk is applied with accuracy to this leather. Before it is fastened on, it is heated, and besmeared first with butter of cacao, then with a large quantity of Kienmayer's amalgam*; and after it is fastened on, it is compressed in conjunction with the wood, or pressed strongly against the machine. The leather is next covered with amber varnish, amalgam is spread over this, and after the varnish is dry, it is smoothed with a burnisher. This is repeated several times. The whole being very dry, and the rubber being pressed so as to touch the glass in all points, the leather coated with amalgam is covered with a piece of fine white paper, as long as the leather, and half an inch broader, so as to cover the seam that fastens the silk to the leather, and the paper is fastened to the wood above or below, accordingly as it is on the ascending or descending side of the plate.

The rubber covered with fine white paper.

What led to this.

Its advantages.

The glass not rendered dull,

does not become streaky,

and neither it nor the silk is soiled. Cleanliness of the whole machine advantageous.

Dry paper is known to be capable of acquiring a high state of electricity, which induced me to try this substance as an immediate rubber. The following are the advantages, that by my experiments, repeated and varied in a great number of ways, I have found paper employed as a rubber to possess over every other known substance.

1. The glass is not rendered dull by the friction, as happens in length, and by frequent using, when it is in immediate contact with the amalgam.

2. By the immediate contact of the amalgam, the glass frequently contracts streaks here and there, that occasion a circulation of the fluid. This cannot take place in the construction I propose.

3. Neither the glass nor the silk can be soiled. It is well known, that the cleanliness of the glass, as well as of the rubber and the whole machine in general, is of importance in

* I add to this amalgam as much silver, as the mercury can dissolve in conjunction with the zinc.

producing an intense degree of electricity. It is true, that it has been proposed to apply the amalgam to the glass instead of the rubbers; but the greater effect, that seems to be produced by this last method, is only apparent, and consists entirely in the circulation of the fluid on the glass, while far from exciting or accumulating more of the fluid, this process and the circulation disperse it.

Rubbing the plate with amalgam instead of the rubbers a bad method.

4. The amalgam on the leather does not require to be frequently renewed. The dust of the amalgam, that is deposited on the edges of the paper, is injurious only when accumulated there in sufficient quantity to be conveyed to the glass, from which however it may easily be removed.

The amalgam does not require frequent renewing.

5. The return and passage of sparks to the rubbers are rendered more difficult, as the paper sufficiently covers the borders of the rubbers, that are turned toward the axis.

The return and passage of sparks to the rubbers more difficult.

6. In my construction the rubbers may be larger than in the usual way, and in reality they are larger in proportion in my machine than in Van-Marum's. No spark passes the axis, unless the air be very damp; for the fluid, in case of a strong accumulation, flies in preference to the opposite rubber over a quarter of the periphery of the glass. I am persuaded, that, by adopting my construction, the rubbers of a plate of 32 inches, such as Van-Marum's is, may be eleven inches instead of nine, in which case there would still be two inches for the diameter of the piece of wood that fastens the plate to the axis, and three inches for the distance from this piece to the rubbers; which I think would be sufficient in these circumstances; and the friction being on a larger surface of the plate, the effect must naturally be much greater. I shall try this alteration of the rubbers on large plates of Bohemian glass, as well as on English cylinders of 18 inches diameter, and 21 inches long. The result I have already obtained with a small cylinder gives me reason to hope much more complete success with a large one.

The rubbers may be longer.

Sparks do not pass to the axis.

Farther trials intended.

7. With my rubbers the friction may be rendered much greater, than with those the amalgam of which is in immediate contact with the glass, and soils it; besides, the plate turns with an uniform friction.

The friction may be made much greater, and is uniform.

8. The activity of the machine is extraordinarily increased by this construction. The greater freedom with which the plate

The activity of the machine extraordinarily increased, and why.

plate moves, even under a greater pressure, and the paper's preventing the glass from being soiled, would be sufficient to produce this effect; even if the greater pressure alone did not occasion a more powerful effect than can be obtained from common machines.

IX.

On the Cultivation of the Sunflower, and its Advantages.*

The sunflower
recommended to
cultivators.

THE sunflower, *helianthus annuus* L. is said to have been brought to Europe from Peru. There are two species, one annual, the other perennial: of these the latter is only ornamental, but the annual is of such use in agriculture, that it deserves to be made known to our readers.

Seeds,

have two va-
rieties.

The seeds are white, gray, or blackish; but this difference of colour is entirely accidental, as seeds of either colour produce the others reciprocally. There are two varieties, however, that appear to me permanent, the sunflower with a single stem, and that with a branchy stem, which is less common.

Method of cul-
tivation.

The seed should be sowed in spring, when nothing is to be apprehended from the frost, that would destroy the young plants. The ground should be well broken and manured, if you would have an abundant crop. You may sow broadcast, and afterward thin the plants: but it is better to dibble the seed, placing two in a hole, the holes a foot distant, and the rows two feet asunder. If both seeds germinate, the weaker plant should be pulled up. The plants should be weeded, and the earth dug up between the rows at a proper time. The height of the plant will be from six to nine feet: the stalks are large, some of them being seven or eight inches in circumference near the ground. It flowers in July or August, and the seeds are ripe in Autumn, at the same time with Indian corn. Rainy seasons destroy many of the plants: the foot of the stalk rots in the ground, the leaves suddenly dry up, the stalk breaks off at the root, and the plant dies. A few sunshiny days stop the progress of this disaster.

Rainy seasons
bad,

* *La Decade Philosophique*, No. 26. June, 1803. p. 507.

The leaves of the sunflower furnish abundance of agreeable fodder for cattle: they are gathered in succession without any perceptible injury to the plant; and after this crop of excellent fodder, you may expect another of seeds, that is very abundant. Some stalks afford not less than ten thousand. The best mode of gathering them is to cut the flower stalks, and, as the calyx is very thick, to hang them up in an airy place, that they may dry speedily.

The leaves are excellent fodder.

The Mode of gathering the seed.

When they are in flower bees flock to them from all quarters to gather honey.

Bees fond of the flowers.

The seed is rather farinaceous than oily, which they who have attempted to express oil from them did not consider. It is true, that oil may be extracted from them, but in quantities too small to make it worth while to cultivate the plant for this purpose.

The seed affords oil, but in small quantities.

But if the seeds of the substance be incapable of affording oil with advantage, they are valuable for feeding animals commonly kept in the country. They are perfectly well suited to sheep, pigs, and other animals; but they produce a more striking effect on poultry. For these no food is more profitable, or occasions them to lay more eggss.

Good food for sheep, pigs, &c.

particularly for poultry; promoting their laying.

The dry stalks burn well, and afford very good ashes for lye, because they contain a great deal of alkali. In short, from the ease with which it is cultivated, the abundance of its produce, and the change it makes in crops, the sunflower may be considered as a new source of wealth to the farmer.

The stalks burn well, and afford much alkali.

X.

Chemical Examination of a new Vegetable Salt, and of a new Acid discovered in it: by KLAPROTH.*

THE vegetable production, which forms the subject of the present examination, affords a fresh proof, that hot climates have not only a great influence on the elaboration and particular modification of a great number of vegetable substances, but that they cause productions to be formed, the least vestige of which is not to be found in the same plants in the northern regions.

Hot climates modify the vegetable products of cold, and create new ones.

* Scherer's *Allgemeines Journal der Chemie*, 1803. No. 55. p. 1.
VOL. VII.—FEBRUARY, 1804. K This

A saline mass exuded from the trunk of a mulberry tree in Sicily.

This production consists of a saline mass, excluded from the trunk of the white mulberry, *morus alba* L. which was observed and gathered by Mr. Thompson, in the botanical garden at Palermo.

External characters.

The saline mass in its native salt, as found on the surface of the bark, has the appearance of a coating in little granulous drops of a yellowish and blackish brown. The substance of the bark is equally penetrated with this salt.

Taste.

The first property by which it is distinguished is its taste, which comes nearest to that of the succinic acid.

Effect of heat.

On burning coals this salt swells up slightly, emitting a vapour scarcely perceptible to the eye, but irritating the organ of smell, and leaves a slight earthy residuum.

Lixivation affords a light, pale salt, in-radiating needles; not deliquescent.

To separate the salt, six hundred grains of bark loaded with it were cut off, and lixiviated with a sufficient quantity of water. The lixivium, which was of a brownish red colour was filtered, and evaporated for crystallization; when three hundred and twenty grains of a light salt were obtained, resembling in colour a pale wood, and composed of short needles united in radii, and not attracting the moisture of the air.

Little soluble in water, though soon crystallizable.

Though these crystals do not form till the lixivium is greatly condensed by evaporation, the salt, nevertheless, belongs to the class of those that are little soluble; for 1000 parts of water dissolve only 35 parts with heat, and 15 parts cold.

Contains no sulphuric acid.

The solution of this salt was not rendered turbid by the acetite or the water of barytes, which proves, that it contains no sulphuric acid. The alcalinule carbonates precipitate from it, an earth of a brown wood colour, which by a slight heat is calcined to whiteness. This earth dissolves with effervescence in nitric acid, and was precipitated from this solution in the form of sulphate of lime by means of sulphuric acid; and, in that of oxalate of lime by oxalic acid.

Precipitates acetite of lead, and nitrate of silver and mercury.

The solution of the salt readily precipitates acetite of lead, and the precipitate formed is reduced on burning coals. The nitrate of silver was precipitated by it in a pale brown, light, and shining scales, and the nitrate of mercury in whitish flocks.

Hence it contains lime, with a peculiar vegetable acid, and extractive matter.

These experiments indicate, that the salt in question is a neutral salt, composed of lime and a peculiar vegetable acid. This acid, however, was combined with an extractive matter, which rendered the precipitates, that otherwise would have been white, either of a deep or pale wood colour.

Fifty grains of the calcareous salt were heated to incandescence, in a retort communicating with the pneumatico-chemical mercurial apparatus, and twelve cubic inches of hydrogen gas mingled with carbonic gas were obtained; that burnt with a strong flame. The bulb of the neck of the retort contained an acid liquor, on which swam a fluid brown oil, both together weighing seven or eight grains. The residuum in the retort consisted of a spongy conglobulated mass of a pale brown colour, interspersed with coally particles, and dissolved in nitric acid with effervescence. This solution, being filtered, was precipitated with carbonate of ammonia; when 21 grains of carbonate of lime were separated. The coally matter that remained on the filter was very light, weighed four grains and half, and burned on a test, leaving a residuum of a quarter of a grain of calcareous earth.

By strong heat it gave hydrogen and carbonic gases,—

an acid liquor, and a fluid brown oil, having a spongy residuum, containing lime, and a carbonaceous matter.

Another portion of the calcareous salt was precipitated by carbonate of ammonia, and the liquid remaining was evaporated by a gentle heat to crystallize it, when it furnished a salt in long and slender prisms.

By carbonate of ammonia, a salt in long slender prisms was obtained.

The liquor left, after the crystals were separated, was examined by several metallic re-agents. The solutions of silver, mercury, copper, iron, cobalt, and uranite in nitric acid, and those of lead and iron in acetic acid, were powerfully precipitated by it. The precipitate of copper was of the colour of verdigris; that of cobalt of a pale reddish colour; that of uranite of a yellowish colour; that of iron of a dull brown; and those of silver, mercury, and lead, of a bright brown wood colour.

The remaining liquor precipitated various metallic solutions.

The same liquor was rendered slightly turbid after a time by the water and acetite of barytes, the muriate of tin and gold, and the nitrate of nickel; but these precipitations might be the effect of the extractive matter, that adhered to the acid, rather than of a combination of this with the metallic oxides.

And the re-agents, perhaps owing to the extractive matter, rendered it slightly turbid.

I shall proceed to the experiments I made with a view of obtaining the acid of the calcareous salt pure.

Experiments to obtain the acid pure.

Forty-five grains of the precipitate obtained from the decomposition of the calcareous salt, by the acetite of lead, were mixed with a scruple of sulphuric acid diluted by a drachm of water. The sulphate of lead was separated, and the liquor evaporated. It afforded by crystallization thirty-four grains of acid salt in fine needles of the colour of pale wood.

The precipitate with acetite of lead mixed with sulphuric acid. Decomposed by sulphuric acid.

Decomposed by
Sulphuric acid.

In the same manner thirty grains of the dry calcareous salt were decomposed by twelve grains of sulphuric acid, properly diluted. The sulphate of lime being separated, the same acid salt was obtained.

Has the taste of succinic acid ; is not deliquescent, dissolves readily in alcohol and does not precipitate metallic solutions.

The taste of succinic acid is still more marked in the acid itself. The acid salt remains dry in the air ; it is easily soluble in alcohol as well as in water. It does not precipitate the metallic solutions like its salt.

In the retort an acid liquor came over ; prismatic, colourless, transparent crystals sublimed ; and a coal remained.

Twenty grains of the acid salt were slightly heated in a small glass retort. First, a couple of drops of an acid liquor came over, the taste of which was perfectly analogous to that of the concrete acid. Next a concrete salt arose, that adhered flat against the top and part of the neck of the retort, in the form of prismatic crystals, colourless and transparent. A coaly residuum remained in the retort.

The acid adheres strongly to the lime.

A similar saline sublimation was not observable with the calcareous salt : we must therefore conclude, that the acid adheres strongly to the lime, and cannot be separated from it by heat, without being decomposed.

The acid washed out and crystallized.

To separate the sublimed salt from the coaly residuum, the whole contents of the retort were dissolved, and the liquor filtered. The solution was perfectly clear, and by spontaneous evaporation deposited the acid salt in colourless crystals.

Sublimation the best mode of obtaining it pure.

From this it appears, that a gentle sublimation is the best mode of obtaining the pure acid salt, and freeing it from the extractive matter, to which it adheres too strongly to be separated from it in the moist way.

Perhaps may be obtained from mulberry trees in this climate.

The small quantity of the calcareous salt I had left, did not allow me to carry the examination as far as I could have wished, in order to determine the specific characters of the acid. It is possible, that the mulberry trees in this climate may likewise furnish the same salt, and I shall proceed to inquire into this without delay.

A new acid, the moroxylic ; the salt moroxylate of lime.

The experiments related, however, seem sufficiently to establish the point of the acid obtained, being a new vegetable acid, which comes nearest to the succinic, both in its taste and other qualities. We may therefore provisionally give it the name of *moroxylic acid*, and the calcareous salt containing it that of *moroxylate of lime*.

XL.

On Mr. WOOLF's Invention for equalizing the Action of a Crank, in contradistinction to the Effect of the Fly, in producing an uniform Effect against a variable Resistance. In a Letter from Mr. T. JONES.

To Mr. NICHOLSON,

SIR,

I BEG leave to second Mr. Farcy's request for an explanation of the substitute for the fly and equalization of the action of the steam engine, in producing a rotative motion, Inquiry respecting Mr. Woolf's substitute for a fly. given in your valuable work for November last: or, I shall esteem it a favour if you can direct me where I can see it at work*. It appears to me, that the inventor has lost sight of the value of that organ which he intends to supersede, and that he has not applied it in cases, where a machine is required to produce an uniform effect on a variable resistance, in which case the mere equalisation of the action of the power on a revolving crank would have no value at all.

The best attempt that I have seen to unite these two important points, is in a steam engine erected by Mr. Hornblower, at Messrs. Meux and Co's. brewery, where the action of the power on the pin of the crank, may have been in the ratio of the sines of each respective arc in its revolution round the arbor of the fly, I say, "*may have been*," and I wonder it was not, since that mode of communication would have so easily admitted it,—theoretically at least; then indeed, a very light fly would serve to turn the crank past the upper and nether part or points of the circle; but it could not have answered in regulating the unequal resistance which is found even in grinding malt, or pump works; and in my humble judgment, the fly must for ever keep its place and importance with every judicious mechanic.

I am, in much bodily indisposition,

SIR,

Your humble servant,

T. JONES.

Surry Road, 7th January 1804.

* If there be one at work near London, I have no doubt but Mr. Woolf the inventor, who is resident engineer at Messrs. Meux's brewery, will readily shew it. N.

Farther

Farther Remarks on Mr. WOOLF'S Rotatory Apparatus.

Farther account
of Woolf's rota-
tory apparatus.

IN order to render the effect of Mr. Woolf's contrivance (Fig. 1. Plate XI. Vol. VI. November 1803.) more intelligible to my readers in general, I shall here explain the same in a popular way.

I will suppose, by way of simplifying the subject, that the rod B is so long as to act always nearly in the perpendicular, and that the action upon the small wheel is also in the same direction, namely, constantly that of gravity. The engraver has omitted to continue the rod of the piston supposed to work in the barrel G, and the proof came too late for it to be altered in time. The effect at the pin F in the little wheel, is precisely the same as if a weight was continually hanging there.

Effect of the
power and
weight in diffe-
rent positions.

1. In the position here shewn, the power of the engine does not act at all, and the weight at F has its full operation. —2. When C has moved through half the quarter circle to the right hand, F will have arrived at the lowest point; consequently the power of the engine will act by a lever equal to the sine of 45° , or seven-tenths of the radius; or seven-tenths of the full power of the engine will then act on the great wheel, and the weight at F will not act at all: —3. When C has moved through an entire quadrant, the power of the engine will be the greatest possible on the great wheel, but at the same time the weight will have arrived at the horizontal line beneath E, and will oppose the former action. The motive force will therefore be equal to the excess of the power of the engine beyond the weight. 4. When C has moved through another half quadrant, the action on the great wheel will be equal to seven-tenths, as in No. 2. and the weight, having arrived at the highest point will not act at all. 5. When C has moved through half a circle, and is at the lower station in the great wheel the engine will not act, and the weight having arrived again at F. will have its full operation as in No. 1.

There are four
positions in
which the coun-
ter-weight has
no effect;

By pursuing this simple method of examination, we see that 1. there are four positions in which the arm C standing at an inclination of 45° to the horizon, and the arm F perpendicular to the same, the regulating weight can have no effect,

effect, whatever be its measure; the action will be effectual to seven-tenths of the direct or entire action of the arm B in the tangent to the great circle; 2. that when the arm C is either perpendicularly up or down, the regulating weight alone acts in the same tangent; 3. that when C lies horizontal, the effective force will be the excess of the power of the engine beyond that of the weight. Since therefore we can do nothing with regard to the four first mentioned positions, we have only to consider the four last, and to contrive that the moving force shall be the same at the top and bottom, as at the two sides; or in other words, that the weight alone shall be equal to the excess of the power of the engine beyond that weight; which is the same thing as saying, that the weight must be equal to half the power of the engine.

two in which the weight acts by itself; and two in which the effect is produced by the excess of the power beyond the weight.

The weight must be half the power.

We see then that the action on the wheel C, will vary from seven-tenths to five-tenths, four times in every revolution; the extreme difference being therefore two-tenths each way from the medium, which is about six-tenths. It may not perhaps be of any practical value to discuss the gradation by which the increase and diminution of the opposite actions are governed; and the mathematical reader will easily see that the direct action of each is as the cosine of the angle of the obliquity of the radius it acts upon. Still less does it seem to be of any consequence in the present loose view of the subject, to treat of any assumed obliquity of the rods of communication.

Variation one tenth on each side the medium action,

If we suppose a power to act constantly and uniformly, but with alternations of opposite direction, in equal times and through like spaces; and wish to produce, by that action, a rotatory motion, we might in theory obtain the effect by a double spiral, known by workmen by the name of a snail: but the difficulties from friction, Snail, shake, and other impediments, would in many instances render the construction ineligible. The crank is the most cheap, easy, light, and practicable means of producing rotation from alternate right-lined motion and the contrary; and its properties are too well known to require discussion in this place. Its inequalities demand a fly, whether the power or the resistance be uniform or variable; and I do not see any striking difference between Mr. Woolf's contrivance

Concerning the equalization of actions, &c.

Crank,

trivance and the fly, as far as concerns one single revolution, whether they be considered as regulating any one of these. In both, the surplus power is employed in putting a weight into a situation or state by which it is enabled to give back (in theory) the same surplus, when the resistance becomes greater or the power less. But if the whole power, in several successive revolutions, should exceed the resistance, and produce acceleration, the fly will continue to accumulate momentum, and will be ready to give it out, against an augmented resistance in succeeding revolutions; so that its equalizing power extends not only through each single revolution, but through many. I do not perceive this property in the ingenious contrivance before us.

XII.

Observations on the extinct Volcanoes in the Environs of Coblentz, by the Chevalier DE SADE. Translated from the original Manuscript, communicated by the Count de BOURNON.

Coblentz built of volcanic stone,

from the neighbourhood of Andernach.

The Roman legions said to have been driven from their camp near Andernach, by a volcano.

This questionable.

The extinct volcanoes, are like those of Auvergne, of unknown date.

Beyond Andernach, river sediment intermixed with pumice stones and volcanic sands. Agerberg, a small mountain on the west.

WHEN I resided at Coblentz in 1791, the volcanic stones made use of in that city, led me to inquire whence they were procured. I was soon informed, that they came from the neighbourhood of Andernach; and the clergyman of that place acquainted me, that the extinct volcanoes were near Haach, an abbey of Benedictine monks, three leagues distant. This worthy gentleman added, that he had somewhere seen it mentioned, that the Roman legions, encamped near Andernach, were obliged to quit their post, on account of the fire issuing from the volcano. Its great distance from the sea, made me doubt the truth of this; and my own observations have convinced me, that the extinct volcanoes of this country are at length as ancient as those of Auvergne, the date of which is unknown.

Having procured a guide, and directing my steps toward the south, I had scarcely quitted Andernach, before I met with river sediment, intermingled with pumice stones and volcanic sands. Leaving the high road, I proceeded toward a small mountain on the west, called Agerberg.

To

To my great satisfaction, after having traversed pumice stones and volcanic scoriæ, I arrived at the foot of a quarry of lava, from which hewn stones for building and millstones were taken. This quarry was wrought in the open air, and was covered with a bed of mould about eighteen feet deep. The thickness of this stratum demonstrates the high antiquity of the volcano, while at the same time it prevented me from tracing the course of the lava, particularly as above this lava, nothing is to be found but scattered blocks and fragments of lava, scoriæ, and pumice stones. I went to the top of the mountain, however, in hopes of discovering where the center might have been; for the mountain of Agemberg, all the rocks of which are torrefied, and the state of scoriæ, was not a crater. The mountains, by which it is surrounded on the west, appeared to me to be of the same nature, without any indication of the mouth from which the fire issued.

The summit of mount Agemberg is very bare, but on descending a few paces, I found a very pleasant copse. A narrow path, which I took at a venture, led me, after some twenty steps, to a grotto, in which was a tomb, with an inscription from Job, reminding man of his nothingness. By the side of this grotto is another, inhabited by a hermit, with a table of volcanic stone in the centre, which he uses for different purposes. A little higher up, I discovered a third, smaller than either of these, containing a rustic altar; on one side was a pigeon-house, and lower down, two or three little basons of water bordered with shells.

I quitted this mountain with regret, to traverse another, which, though covered with wood, seemed to indicate the existence of a crater; but I could perceive no trace of one, the trees, and the thickness of the mould, letting me see nothing but scattered blocks of lava and scoriæ.

My examination was soon finished, and I resumed the road to the abbey of Haach. In this I found nothing but pumice stones, lava; in some places of the basaltic kind, scoriæ, a few fragments of hornstone schistus, and blocks of quartz; all scattered about on a stratum of earth, which itself was composed merely of river-sand and pumice stone.

The

A quarry of lava, from which hewn stones and millstones were procured.

Wrought in the open air, and covered with eighteen feet of mould.

Above it scattered blocks and fragments of lava, scoriæ, and pumice stone.

Agemberg not a crater, and the mountains, surrounding it, of the same nature.

The top of Agemberg bare. A little way

down a pleasant copse, in which is a grotto, containing a tomb, another inhabited by a hermit, a third in which is an altar, a pigeon-house, and two or three basons of water.

Another mountain covered with trees and deep mould, with scattered blocks of lava and scoriæ.

In the road to the abbey of Haach, pumice stones, lava, in some places basaltic, scoriæ, hornstone, schistus, and blocks of quartz, on a stratum of river sand and pumice stone.

The crater said to be a lake near. Its situation fine.

On the west side first argil of different colours; then hornstone, next scattered blocks of lava.

The bank a poor meadow.

On the north, blocks of lava, then hornstone cleft perpendicularly, and lastly, a fat, white clay, interspersed with blocks of lava and hornstone.

On the east, compact and porous lava, and the mountain nearly perpendicular.

On the south a level meadow, and a shore covered with shells.

The prevailing winds N. and N. W.

The lake has no apparent influx of water, but a rivulet runs from it on the south.

Varies greatly in depth, more than 400 feet near the middle. Abounds with fish, but the bottom too rocky to use nets.

The abbey built by the count Palatine in 1093, he being alarmed by the nocturnal appearance of fires on the mountains.

The incumbent of the village belonging to the abbey, informed me, that the crater, of which I was in search, was a lake near his parish.

The situation of this lake displays much grandeur. It is surrounded by a chain of mountains covered with trees; its shape is oval, longest from north to south; and it requires near two hours to walk round it.

The west side offers to view at first nothing but a spacious bed of argil, divided into bands of different colours; after this appears a smaller bed of hornstone; and the rest exhibits only some scattered blocks of lava. The bank is an indifferent meadow.

The north side begins with some of the blocks of lava just mentioned: next appears a pretty extensive bed of hornstone, the very numerous fissures of which are uniformly perpendicular to the horizon: and a little farther on, is a considerable bed of fat clay, as white as snow, interspersed with blocks of lava and hornstone.

On approaching the east shore, nothing is to be found but lava, in some places compact, in others porous, and the mountain there is nearly perpendicular.

Lastly, on the south side, the ground becomes level, and is simply a meadow. The strand is covered with shells, partly whole, partly broken; and hence I conclude, that the prevailing winds in this district, are the north and north-west.

This lake does not appear to me to receive water in any part: yet, on the south it has an outlet, that furnishes a small brook, watering the grounds of Medermich. The depth of the lake varies greatly, being upwards of four hundred feet near the middle. It abounds with fish, particularly pike; but the blocks of stone scattered over its bottom, render the use of the net nearly impracticable, so that the line is almost exclusively employed for catching them.

The abbey, to which this lake belonged, was a spacious building, proclaiming the opulence of its founder. It was built in 1093, by Henry count of the Palatinate of the Rhine, and lord of this lake. Broverus, in his annals of Treves, says, that the phantoms and lights, which this prince saw every night on the mountains around him, induced him, to erect this pious foundation.

I was told, that the outlet I have just mentioned was artificial, and made fifty years after the building of the abbey. The present outlet artificial,

The foundations of this abbey, having been sunk as deep as the outer walls are high above ground, had obstructed the natural outlet, which occasioned the water to rise so high, as to inundate the convent, and render it necessary to form the present channel, in order that it might run off. the natural one having been obstructed by the foundations of the abbey.

The face of the ground, however, makes me doubt, whether the lake ever flowed over on the side next the abbey; for to a considerable distance it rises there uniformly as you proceed from the shore. It appeared to me, that This questionable,

the natural outlet must have been rather on the side toward Andernach, through fissures on the mountain. Neither can I believe, that this lake, if we may judge from its banks, was the crater of a volcano; but one of the great excavations produced by some earthquakes. In fact, I have already said, that the east and north-east parts of this lake were volcanic: if therefore we suppose, that the summit of the mountain had been a volcano, its base must necessarily have become hollow, and in this state of things, a very slight shock would have sufficed to break the arch of the vault, or the crust that covered its abysses. as its natural outlet was more probably toward Andernach, through fissures in the mountain. Neither was it a crater, but probably produced by an earthquake.

We need not wonder, that this lake is always full of water, though it appears to receive none in any part; since it serves as a drain to the surrounding mountains, which are in great measure volcanic. Its water is very limpid, and it does not freeze except in very severe frosts. Marquart, in his origin of the Palatinate, asserts, that it was once larger than it is now; which in fact is perceptible on the south and west, where the ground has the appearance of having been once covered with water. He adds, that precious stones and sapphires were found on its borders. I did not see any; but it is known, that there is a fact of this kind in the little rivulet of Pezzouliou, that runs among lava at Expilly, near Puy, in Velay. Receives water from the surrounding mountains. Very clear, and does not easily freeze. Formerly larger than it is now. Sapphires and other precious stones, once found on its borders. A similar occurrence at Expilly.

By no means satisfied with my researches, the principal object of which was, to discover the crater of the volcano, that could have produced all the lava in this country, I repaired to Medermenich, half a league to the south-east. There I found myself in quarries of volcanic stone, from which were taken mill-stones and hewn-stones for building, At Medermenich, quarries of volcanic stones.

for which there is a very great demand. These quarries are underground, and in these are shafts a hundred feet deep, to facilitate the working.

Depth of the strata.

The first stratum was mould, about 15 feet thick.

The second, scorix of lava, about 12

Total 27.

Descent into them.

Beneath was the pure and compact lava. The descent into these quarries is by a stair of 120 steps, and a gentle descent of near 90 paces. The fragments of the stones got out served to make internal walls of support, to which were added stanchions of timber, in some places perpendicular, in others oblique, but always contrived to support the blocks separated by fissures. The stones were broken off by iron wedges, for they were afraid to separate them by blasting.

The roof supported by walls and timber.

The stones separated by iron wedges.

The shafts numerous, and very dangerous from being left open.

These quarries wrought for several centuries.

I counted eighteen or twenty shafts in the space I traversed; and their mouths being perfectly open, woe to the traveller who should lose his way among them by night. I observed several of them covered up, indicating works abandoned; whence we may presume, that these quarries had been wrought for several centuries. Some old oaks likewise growing in the shafts thus covered confirm this opinion.

Its crater probably at mount Blemberg.

Beyond Medermenich a bed of the same lava, with a perpendicular face, forming a ravine

25 feet deep, and covered with a thin stratum of mould.

This too wrought, and the stone harder from its exposure to the air.

Beyond this, and some lava of the piperino species, is a cold acidulous chalybeate spring.

More lava in which were formerly quarries,

The thickness of the strata covering this lava prevented me from following its course, and tracing it to its source; but I judged, that the mountain of Blemberg, a league to the south-west, might be its crater. Accordingly I proceeded to examine it. A quarter of a league from Medermenich I found a bed of the same lava, the face of which was perpendicular, and forming a gully, that might be twenty-five feet deep. It is covered by a very thin stratum of mould, and is wrought into millstones, and stones for building. Being exposed to the open air, it is harder than that of Medermenich.

After having passed this gully, and a little bed of muddy lava, of the nature of piperino, we find in a meadow a very copious mineral spring. It is cold, and contains carbonic acid and iron. At some distance from this we meet with beds of lava, in which there had anciently been several quarries, and at length we arrive at the mountains of Blemberg. The surface of this mountain, though covered with trees on the east, is nevertheless full of torrefied and scorified lava, and gray and red pouzzolona, which left me no doubt, that this mountain had formerly been a volcano. The inspection of its summit

confirmed me in this opinion. Though divided into several parts, which do not perfectly correspond with each other, that on the west perfectly represented a segment of a vast crater in good preservation. Standing on the border of this segment, I observed on the east, at the distance of half a league, quarries of lava, of which this mountain was the source. The gully I have mentioned seemed to militate against the adoption of such an opinion with respect to the quarries of Medermenich, which are to the north-east beyond these: but time, and the various circumstances, that have formed fifteen feet depth of mould, may have occasioned the apparent interruption of this current of lava. I do not doubt however, but several of the neighbouring mountains have been openings of volcanoes.

From the summit of Blemberg I enjoyed a grand and very extensive prospect. On the south I had the little town of Mayerne, on the north Medermenich, and Ettringen on the west.

I quitted this mountain to visit that of Zimmeray, about half a league west of it, and I passed through the village of Ettringen, built of lava and scoræ.

The whole of the body of the mountain of Zimmeray is volcanic. The lavas there are in a state of torrefaction, that announces the existence of a crater. In fact, when I reached its summit, I there found all the marks of one. It is of a conical form, and has a hollow opening to the south. The sides of this hollow facing the south are well preserved: those that face the north have been destroyed. I imagine this took place at the period of the last eruption, and that a stream of lava would be found by digging on that side.

I then left this mountain, and proceeded to that of Calberg, which is on the north, and separated from it only by a tolerably broad valley, in which I found trapp, of a blackish gray colour, in mafs, and in scattered blocks. At the foot of mount Calberg was a bed of black, coarse grained, volcanic sand, stratiform, and containing a great deal of mica in large leaves. The trees that covered its summit prevented me from ascertaining its nature, but the circumference of its base is entirely volcanic. Around it is muddy lava, in which larger or smaller fragments of compact lava occur. Gray clay predominates there.

Blemberg covered with trees on the east; but abounds with lava and pouzzalana.

Its summit once a vast crater, but partly destroyed.

Quarries of lava that proceeded from it.

As probably did those of Medermenich.

But several of the neighbouring mountains were once volcanoes.

Prospect from Blemberg fine.

Village of Ettringen built of lava and scoræ.

The mountain of Zimmeray volcanic.

Its summit a crater.

Mount Calberg contains trapp.

At its foot a bed of volcanic sand, containing mica in large leaves.

Its base entirely volcanic.

Gray clay predominates.

Once a volcano. I had no doubt, that this mountain was once a volcano, its figure alone proves it. I went beyond it to the quarries that were working, and which were to the north, at the foot of a

Near it quarries of lava. of the piperino species; gentle declivity, separated from the Calberg by a valley, through which a small brook flowed to the east. These quarries are parts of vast beds of that kind of lava, which the Italians call piperino. It is in general an earthy lava, containing a pretty considerable quantity of fragments of compact

Soft and easily wrought; in some places a volcanic breccia, in others of a sandstone texture.

Road from Medermenich to Coblenz.

The plain covered with fragments of quartz, trapp, and pumice stone. Near Oetundung slate quarries. Near it four volcanic mountains.

Their lava encrusted with small crystals of piroxen.

It is reddish, and contains reddish mica in large leaves. Used in walls.

View from these heights,

lava. This stone is soft, and easily wrought. It is almost every where in a horizontal stratum, but varies in its texture, for in some places it appears in the form of a volcanic breccia, in others it has the texture of sand-stone.

Having accomplished my object, which was to discover the volcanic openings, that furnished the lava of this district, I returned to the Medermenich, where I slept; and the next day I reached Coblenz. The plain I crossed was covered with fragments of quartz, trapp, and pumice stone. Near Oetundung, on the high road, we find rocks of slate, which are working. After leaving this village four gently sloping mountains appear on the left, stretching from east to west, and separated only by narrow valleys. Their figure induced me to visit them; and I found them composed of pumice stones and scorice. The first and second are merely segments of two small craters, the northern part of which has been carried away. The other two, which are covered with trees, appear to me to have formed but one crater of vast extent, the southern part of which is destroyed. The lava of these little mountains is encrusted with an immense number of little crystals of piroxen. This reddish lava contains a pretty considerable quantity of reddish mica in large leaves, frequently half a line thick. Almost at the foot of the easternmost mountain this stone is procured, to be used in constructing walls, though it is too porous, and at the same time too hard, to be employed as hewn stone.

From the summit of these heights the prospect is beautiful and grand. From the east to the north appears the magnificent basin watered by the Rhine from the castle of Ehrenbreitstein to Andernach. The seven mountains (Siebenberge) raise their ancient heads on the north. On the west, the view includes from north to south all the mountains just described;

and

and on the south it is terminated by mountains covered with wood, the nearest of which appeared to me to be volcanic.

At length I arrived at Bafenheim, remarkable for nothing but the English gardens belonging to the lord of the manor; and thence to Coblentz. I found pumice stone on the road, and in strata where pits had been dug in the ground.

English gardens at Bafenheim.

Pumice stone on the road and in strata.

XIII.

On an Improvement in the Form of Spectacle-Glasses, by WILLIAM HYDE WOLLASTON, M D. F. R. S. Communicated by the Author.

IT must have been remarked by persons who make use of spectacles, especially those who require glasses of short focal distance, that objects seen through them appear distinct, only when viewed through the central parts of the glasses; that when the direction of the sight E O, *Fig. 1.* page 145 is considerably inclined to the surfaces, objects appear distorted, and that this defect is greater in proportion to the greater obliquity of that line.

Imperfection of spectacles, that objects out of center are distorted.

It is on this account that opticians have lately made, and recommended spectacle-glasses of less diameter than those formerly in use, thinking that the extreme parts of the field of vision, which from indistinctness were of little use, might be spared without much inconvenience. But this alteration in the size of the glasses could hardly claim the merit of an improvement; since for one defect, it only substituted another scarcely less objectionable.

Remedy formerly proposed; to make the glasses smaller;—

but to little effect.

It seems indeed rather extraordinary, that, during five centuries which have elapsed since the invention of spectacles, neither theory nor accident should have produced any considerable variation from the original construction.

It was indeed conceived by Huygens, that the glasses, instead of being equally curved on both sides, as is customary, should have the curvatures of their opposite surfaces in the proportion of six to one, because he had demonstrated that such a form was best suited to the object glasses of telescopes.

Huygens proposed that the radii of curvature in spectacle glasses should be unequal.

Dr. Smith also in his treatise on optics, (p. 258.) repeats this opinion of Huygens in the following cursory manner; and

Repeated by Smith.

"and consequently this figure of a glass is the best for spectacles, as the double concave of like figure is the best to help short-sighted persons."

This is good for simple object lenses, but not for spectacles.

But although it may be very true that such a form of glass was best calculated for the object glass of a telescope, previous to the celebrated discovery of the *achromatic* object-glass by the late Mr. Dollond, yet whatever advantages might at any time be expected from the telescopic object-glass so shaped, these were not to be obtained by a similar construction in spectacles, as may easily be seen by considering the different uses of the respective instruments.

Telescopic vision is nearly in the axis:—

In a telescope in the first place, our view is necessarily confined to a very small distance on each side of the axis; and secondly, every part of the object glass contributes to the distinctness of any object viewed.

and the whole lens acts for every radiant point.

It is under these circumstances alone that the proportion of the curvatures above-mentioned, might be proper for a single object glass, as being capable of collecting into the focus the rays that fall on every part of it parallel to the axis.

In spectacles it is very different.

By spectacles on the contrary, objects are to be viewed if possible in every direction in which they might be seen by the naked eye, which is often far removed from the centers of the glasses; consequently a constitution that is calculated to represent correctly central objects alone, cannot be the most advantageous.

A small part of the lens only is employed at once.

In these also, the portion of the glass employed at once is scarcely larger than the pupil of the eye, so that any endeavour to procure the concurrence of all parts of a glass in any one effect is evidently superfluous, and may also be shewn to be prejudicial.

Whence improvement must be sought from different considerations.

It is therefore proposed to remedy the imperfections observable in the spectacle glasses hitherto generally used, upon a principle suggested by this latter consideration, which presents an opportunity by a different construction of rendering objects in all directions distinct.

If the lens be made concave next the eye all the rays may be had to pass, through nearly perpendicular.

The alteration requisite for this purpose, is extremely simple, and easily intelligible. Supposing an eye to be placed in the center of any hollow globe of glass, it is plain that objects would then be seen *perpendicularly* through its surface in every direction. Consequently the more nearly any spectacle

Spectacle-glasses can be made to surround the eye in the manner of a globular surface, the more nearly will every part of it be at right angles to the line of sight; the more uniform will be the power of its different parts, and the more completely will the indistinctness of lateral objects be avoided *.

According to this principle, all spectacle-glasses should be convex on their exterior surface, and concave within. The section of those for long sighted persons, will assume the form of a meniscus or crescent, *Fig. 2.* and those adapted for short sight will have their principal curvature on the concave side, *Fig. 3.*

Convex outside
and concave
within.



Fig. 1

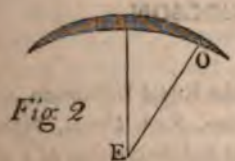


Fig. 2



Fig. 3

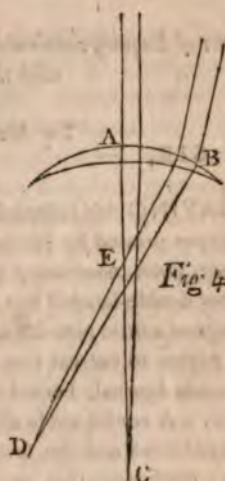


Fig. 4

It is only necessary to add, that the advantage of this improvement in the form of spectacle glasses has been confirmed by a sufficient number of experiments on different persons, and that those in particular who are very long or very short sighted, are much benefited by them.

Experience confirms this doctrine.

* To mathematicians it will be evident that any ray which does not pass through the center of a lens, cannot be at right angles to both surfaces; but they will also perceive that when any small oblique pencil makes equal angles with the two surfaces of a thin lens, the inclination of it to each is so small, that its focal length *B D*, *Fig. 4.* will not sensibly differ from *A C* that of a central pencil.

and a manufactory is established.

The most advantageous proportions of curvature for obtaining the different focal lengths now generally distinguished by certain numbers, have also been duly considered; and the manufacture of spectacles, on this construction, has been undertaken by Messrs. P. and J. Dollond, to whom the exclusive sale of them is secured by patent, and whose well known skill in the construction of optical instruments, ensures to this improvement every advantage of correct execution.

The spectacles are called periscopic.

From the opportunity afforded by these glasses of looking round at various objects in one position of the head, it is thought they may not improperly be distinguished by the name of *Periscopic Spectacles*.

XIV.

Letter of Enquiry concerning a Gun to throw double-headed Shot; with the Editor's Reply.

To Mr. NICHOLSON.

SIR,

Drawing and description of a gun on a paper at the Royal Institution.

HAVING this instant seen, at the Royal Institution, a sheet of paper printed by Nichols, printer, Earls Court, Newport Street, Soho, containing a drawing and description of a gun to throw double-headed shot, I take the liberty of writing this on a page of a little note-book there, which must be my apology for the paper, to entreat that you will observe upon it in your invaluable Journal, because I fear that it is not a safe instrument. A considerable distance is to be observed between the powder fired and the shot to be discharged by it. I have always considered that an interval so kept was the occasion of the bursting of so many guns. Pray is this safe? And should not a line be cut on the arms to which the bullets are attached, to mark exactly the distance of the attaching bar at each extremity from the muzzle, when the balls are equally rammed down. As the knowledge of its safety may contribute to its service if really useful, I hope you will think this application not idle.

I am, Sir,

Your constant READER.

ANSWER

ANSWER. W. N.

THE paper alluded to in the letter of my correspondent, contains an engraving of the shaded section of a gun having a double bore, but only one chamber; from which each bore directly leads: but they diverge from each other in an angle of about eighteen degrees. The charge of powder is to be placed in the chamber, and the two shot, one for each bore, are connected by a bar having two joints or hinges at an equal distance from its middle. By this contrivance, by bending the compound piece at the hinges, the shot can be put into the bores of the gun; and when the explosion blows them out, their divergence causes the bar to become strait, and consequently is expected to do mischief proportioned to the extent of its length. More particular account.

The title of the paper is, "Horizontal Section of a Gun for projecting double-headed Shot, invented by Lieut. Colonel Blaquiere, 22 Dragoons, July, 1803. Recommended to be used against an invading Army on board his Majesty's Gun-Boats;" and in the directions care is prescribed, that the same length of bar shall be left out of each muzzle to prevent accident; and it is stated to be a great advantage, that a considerable distance should be left between the powder and shot, as by this means the powder acquires its greatest possible force. Title of the paper and instructions.

My opinion was asked concerning a gun of this description which another inventor had proposed, some time previous to Midsummer last, to throw chain-shot; and the observations that then occurred were, 1. If there be the least difference between the windages or the blast in each bore, or in the casual impediments, one shot will precede the other. 2. If they go out fairly together, their velocity of separation will be to the direct velocity as the chord of the angle of divergence is to the radius. In the present gun the balls will separate with one-third of the velocity with which they advance. 3. That the violent separation of the shot from each other, will most probably, in either of these cases, break the chain, or the bar, or the joints. 4. Or if no fracture takes place, the sudden check will, by the elasticity of the metal, cause the shot to approach again to each other. 5. To which I may add, that it may be doubted whether the embarrassment of one of these bar-shots preceding the other might not occasion a dangerous obstruction. This gun invented also by another person. Its probable effects.

tion, and that there are many facts which shew, that it is unsafe to permit any considerable expansion of, or space for, the blast of powder, before it shall act on the projectile.

XV.

Prognostics of the Weather, established by long-continued Observation upon the Conduct and Appearances of Birds, Beasts, Insects, Plants, Meteors, the Heavenly Bodies, Minerals, &c. Communicated by a Correspondent.

To Mr. NICHOLSON,

SIR,

Introduction.
Prognostics of
the weather.

IT is a well known general fact, that shepherds and others, whose occupations lie in the open air, do not unfrequently possess the skill of foretelling the weather for considerable periods of time in advance, and that they ground their observations upon the phenomena exhibited by animals, and other bodies exposed to the action of the elements. Some of the maxims on this subject, which possess the sanction of ancient acquiescence, are such as probably would not stand the test of modern scientific examination; but on the other hand, there are many of which the rationale appears not difficult to explain, and others which, from their empirical value, are highly deserving to be studied and made out. I have for these reasons, as well as from a sense of the immediate utility of this knowledge, thought it by no means impertinent to beg that you would oblige the world with the enclosed, which is the best collection of facts I have met with. It is taken from a small pamphlet printed at Edinburgh, without date or the name of bookfeller or author, but apparently many years ago, intitled, *A Succinct Treatise of Popular Astronomy*. If this communication should meet the honour of insertion in your excellent Miscellany, I hope it will be followed by some explanations from your scientific correspondents.

I am, Sir,

Your obliged reader,

R. B.

Signs

Signs of Rain from Birds.

SEA and fresh water-fowls, such as cormorants, sea-gulls, Prognostics of the weather muir-hens, &c. flying from sea, or the fresh waters, to land, Rain from birds shew bad weather at hand: land fowls flying to waters, and those shaking, washing, and noisy, especially in the evening, denote the same: geese, ducks, coots, &c. picking, shaking, washing, and noisy; rooks and crows in flocks, and suddenly disappearing; pyes and jays in flocks, and very noisy; the raven or hooded-crow crying in the morning, with an interruption in their notes, or crows being very clamorous at even; the heron, bittern and swallow flying low; birds forsaking their meat and flying to their nests; poultry going to roost; or pigeons to their dove-house; tame fowls grubbing in the dust, and clapping their wings; small birds seeming to duck, and wash in the sand; the late and early crowing of the cock, and clapping his wings; the early singing of wood-larks; the early chirping of sparrows; the early note of the chaffinch near houses; the dull appearance of robin red-breast near houses; peacocks and owls unusually clamorous.

Signs of Wind from Birds.

Sea and fresh water-fowls gathering in flocks to the banks, Wind from birds and there sporting, especially in the morning; wild geese flying high, and in flocks, and directing their course eastward; coots restless and clamorous; the hoopoe loud in his note; the king's-fisher taking to land; rooks darting or shooting in the air, or sporting on the banks of fresh waters; and lastly, the appearance of the malefigie at sea is a certain forerunner of violent winds, and (early in the morning) denotes horrible tempests at hand.

Signs of Fair Weather from Birds.

Halcyons, sea-ducks, &c. leaving the land and flocking to the sea; kites, herons, bitterns and swallows flying high and Fair weather from birds loud in their notes; lapwings restless and clamorous; sparrows after sun-rise restless and noisy; ravens, hawks and kestrels (in the morning) loud in their notes; robin red-breast mounted high, and loud in his song; larks soaring high, and loud in their songs; owls hooting with an easy and clear note; bats appearing early in the evening.

Signs

*Signs of Rain from Beasts.*Rain from
beasts.

Asses braying more frequently than usual; hogs playing, scattering their food, or carrying straw in their mouths; oxen snuffing the air, looking to the south, while lying on their right sides, or licking their hooves; cattle grasping for air at noon; calves running violently and gamboling; deer, sheep, or goats, leaping, fighting or pushing; cats washing their face and ears; dogs eagerly scraping up earth; foxes barking, or wolves howling; moles throwing up earth more than usual; rats and mice more restless than usual; a grumbling noise in the belly of hounds.

*Signs of Rain from Insects.*Rain from in-
sects.

Worms crawling out of the earth in great abundance; spiders falling from their webs; flies dull and restless; ants hastening to their nests; bees hastening home, and keeping close in their hives; frogs and toads drawing nigh to houses; frogs croaking from ditches; toads crying on eminences; gnats singing more than usual; but, if gnats play in the open air, or if hornets, wasps, and glow-worms appear plentifully in the evening, or if spiders webs are seen in the air, or on the grass, or trees, these do all denote fair and warm weather at hand.

Signs of Rain from the Sun.

Rain, from sun.

Sun rising dim or waterish; rising red with blackish beams mixed along with his rays; rising in a musty or muddy colour; rising red and turning blackish; setting under a thick cloud; setting with a red sky in the east.

N. B. Sudden rains never last long; but when the air grows thick by degrees, and the sun, moon, and stars shine dimmer and dimmer, then it is like to rain six hours usually.

*Signs of Wind from the Sun.*Wind from the
sun.

Sun rising pale and setting red, with an iris; rising large in surface; rising with a red sky in the north; setting of a bloody colour; setting pale, with one or more dark circles, or accompanied with red streaks; seeming concave or hollow; seeming divided, great storms; parhelia, or mock suns, never appear; but are followed by tempests.

Signs

Signs of Fair Weather from the Sun.

Sun rising clear, having set clear the night before; rising while the clouds about him are driving to the west; rising with an iris around him, and that iris wearing away equally on all sides, then expect fair and settled weather; rising clear and not hot; setting in red clouds, according to the old observation:

*The evening red and morning grey,
Is the sure sign of a fair day.*

Signs of Rain from the Moon.

Moon pale in colour, rain; horns blunt at first rising, rain; horns blunt, at or within two or three days after change, denotes rain for that quarter; an iris with a south wind, rain next day; wind south third night after change, rain next day; the wind south, and the moon not seen before the fourth night, rain most of that month; full moon in April, new and full moon in August, for most part, bring rain; mock-moons are the forerunners of great rains, land-floods, and inundation.

Signs of Wind from the Moon.

Moon seeming greatly enlarged; appearing of a red colour; horns sharp and blackish; if included with a clear ruddy iris; if the iris be double, or seem to be broken in parts, tempests.

N. B. On the new moon, the wind for the most part changes.

When the moon, at four days old, has her horns sharp, she foretels a tempest at sea, unless she has a circle about her, and that too entire, because, by that she shews that it is not like to be bad weather, till it is full moon.

Signs of Fair Weather from the Moon.

Moon seeming to exhibit bright spots; a clear iris with full moon; horns sharp fourth day, fair till full; horns blunt at first rising, or within two or three days after change, denotes rain for that quarter; but fair weather the other three quarters; moon clear three days after change or before full, always denotes fair weather. After every change and full, rains for the most part, succeeded by fair settled weather; moon clear and bright, always fair weather.

Signs

Signs of Weather from the Stars.

Weather from
stars.

Stars seeming large, dull, and pale of colour, rain; or when their twinkling is not perceptible, or if encompassed with an iris. In summer, when wind is at east, and stars seem greater than usual, then expect sudden rain; stars appearing great in number, yet clear and bright, seeming to shoot or dart, denote fair weather in summer, and in winter frost.

Signs of Rain from the Clouds.

Rain from
clouds.

In cloudy weather, when the wind falls, rain follows; clouds growing bigger, or seeming like rocks or towers settling on mountains tops; coming from the south, or often changing their course; many in number at north west in the even; being black in colour from the east, rain at night; but out of the west, rain next day; being many like fleeces of wool, from the east, rain for two or three days; lying like ridges about mid-day in the south-west, shews great storms both of wind and rain to be nigh.

Signs of Wind from the Clouds.

Wind from
clouds.

Clouds flying to and fro; appearing suddenly from the south or west; appearing red, or accompanied with redness in the air, especially in the morning; being of a leadish colour in the north-west; single clouds denote wind from whence they come; but if at sun-set, clouds appear with golden edges, or diminish in bulk, or small clouds sink low, or draw against the wind, or appear small, white, and scattered in the north-west (such as are vulgarly called mackerel) when the sun is high, these are signs of fair weather.

N. B. It is often observed, that though the mackerel sky denotes fair weather for that day, yet for the most part, rain follows in a day or two after.

Signs of Rain from the Rainbow.

Rain from
rainbow.

After a long drought, the rainbow denotes sudden and heavy rains; if green be the predominant colour, it denotes rain, but if red, wind with rain; if the clouds grow darker, rain; if the bow seems broken, violent storms; if appearing at noon, much rain; if in the west, great rain, with thunder.

N. B.

N. B. It is observed, that if the last week in February, and the first fortnight of March, be mostly rainy, and attended with frequent appearances of the bow, a wet spring and summer may be expected.

Signs of Fair Weather from the Rainbow.

The rainbow appearing after long rains, denotes fair weather at hand; if the colours grow lighter, fair; if the bow suddenly disappears, fair; if the bow appear in the morning, it is the sign of small rains, followed by fair weather; if appearing at night, fair weather; if appearing in the east, in the evening, fair; if the bow appear double, it denotes fair weather at present, but rain in a few days; if in autumn, it continues fair for two days after the appearance of the aurora borealis, expect fair weather for at least eight days more.

Signs of Rain from Mists.

If mists be attracted to the tops of hills then expect rain in a day or two; if in dry weather, they be observed to ascend more than usual then expect sudden rain; mists in the new moon always foreshew rain in the old; mists also in the old moon denote rains to happen in the new; a misty white scare, in a clear sky, in the south-east is always a forerunner of rain.

Signs of Fair Weather from Mists.

If mists dissipate quickly, or descend after rain, it is a sign of fair weather; a general mist before sun rising near the full moon, denotes fair weather for about a fortnight running. If after sun set or before sun rise, a white mist arise from the waters and meads, it denotes warm and fair weather next day. A misty dew on the inside of glass windows shews fair weather for that day.

Signs of Rain from inanimate Bodies.

Wood swelling, or stones seeming to sweat; lute or viol strings breaking; printed canvas or pasted maps relaxing; salt becoming moist; rivers sinking, or floods suddenly abating; remarkable sparkling of lamps or candles; remarkable halo about the candle; great dryness of the earth; pools seeming troubled or muddy; yellow scum on the surface of stagnant waters; dandelion or pimpernel shutting up; trefoil swelling in stalk, while the leaves bow down.

N. B. A dry spring is always attended with a rainy winter.

Signs

Signs of Wind from inanimate Bodies.

Wind from inanimate bodies.

Winds shifting to the opposite point; sea calm, with a murmuring noise; a murmuring noise from the woods and rocks; when the air is calm; leaves and feathers seeming much agitated; tides high when the thermometer is high; trembling or flexuous burning of flames; coal burning white with a murmuring noise; thunder in the morning with a clear sky; thunder from the north.

N. B. Whensoever the wind begins to shift, it will not rest till it comes to the opposite point; and, if the wind be in the north, it will be cold; if in the north-east colder; if in the south; it brings rain; but if in the south-west more rain.

Signs of Rain ceasing.

Rain ceasing.

The sudden closing of gaps in the earth; the remarkable rising of springs or rivers; if the rain begins an hour or two before sun rise it is like to be fair ere noon; but if an hour or two after sun-rise, it for the most part, happens to continue all day and then to cease; when it begins to rain from the south with a high wind for two or three hours, and that the wind falls, and it still continues raining, it is then like to continue for twelve hours or more, and then to cease.

N. B. These long rains seldom happen to hold above twenty-four hours, or happen above once a year.

Signs of Wind ceasing.

Wind ceasing.

A hasty shower after raging winds is a sure sign of the storm being near an end. If the water ruckles and frequent bubbles arise, or if the halcyon or king-fisher attempts the sea while the storm lasts, or moles come out of their holes, or sparrows chirp merrily, these are all certain signs of the storm ceasing. Both sea and fresh water fishes by their frequent rising and fluttering on the surface of the water, foretell the storm nigh over, but especially dolphins spouting up water in a storm foretell a calm.

N. B. Let the wind be in what quarter it will upon the new moon, it presently changes.

Signs of Hail.

Hail.

Clouds white, inclining to yellow, and moving heavily though the wind be high is a sure sign of hail; if the eastern sky before sun-rise be pale, and refracted rays appear in thick clouds,

clouds, then expect great storms of hail; white clouds in summer are a sign of hail, but in winter they denote snow, especially when we perceive the air to be a little warm; in spring or winter when clouds appear of a blueish white, and expand much, expect small hail or drizzling, which properly is no other than frozen mists.

Signs of Thunder.

Meteors shooting in the summer's evening, or chops and clefts Thunder, in the earth, when the weather is sultry, always foretell thunder is nigh; in summer or harvest, when the wind has been south two or three days, and the thermometer high, and clouds rise with great white tops like towers, as if one were upon the top of another and joined with black on the nether side, expect rain and thunder suddenly; if two such clouds arise, one on either hand, it is then time to look for shelter, as the thunder is very nigh.

N. B. It is observed that it thunders most with a south wind and least with an east.

Signs of Cold and Frosty Weather.

Sea-pyes, starlings, fieldfares, with other migratory birds Cold and frosty, appearing early, denote a cold season to ensue; the early appearance of small birds in flocks and of robin red-breasts near houses; sun in harvest after setting in a mist or broader than usual; moon bright, with sharp horns, after change; wind shifting to the east or north after change; sky full of twinkling stars; small clouds hovering low in the north; snow falling small, while clouds appear on heaps like rocks.

N. B. Frosts in autumn are always succeeded with rain.

Signs of Thaw.

Snow falling in large flakes while the wind is at south; Thaw, cracks appearing in the ice; sun looking waterish; the moon's horns blunted; stars looking dull; wind turning to the south; wind extremely shifting; it is also observed, that, if October and November be frost and snow, January and February are like to be open and mild.

Signs of Drought.

Fair weather for a week together, while the wind is all that Drought, time in the south, is, for the most part, followed by a great drought;

drought; if February be for most rainy, spring and summer quarters are like to be so too; but if it happen to be altogether fair, then expect a drought to follow; if lightning follow after twenty-four hours of dry and fair weather, drought will follow, but if within the twenty-four hours, expect great rains.

Signs of Hard Winters.

Hard winters. A moist and cold summer, and mild autumn, are sure signs of a hard and severe winter; store of hips and haws denote the same; the hazel-tree flowering is ever observed to foretell the same; acorns found without any insect is a sure prognostic of a hard winter.

Signs of Pestilential Seasons.

Pestilential seasons. A dry and cold winter with a southerly wind; a very rainy spring, sickness in summer; if summer be dry with the wind northerly but the autumn rainy and the wind southerly, great sickness is likely to follow; great heats in spring time without winds; roots having a luscious taste, while the wind has been long southerly without rain; and lastly, great quantities of stinking atoms, insects or animals, as flies, frogs, snakes, locusts, &c.

Experiments of the Leech Worm.

Leech worm. Inclose the leech worm in an eight ounce vial glass, three fourths filled with water covered with a bit of linen, let the water be changed once a week in summer, and once a fortnight in winter.

If the leech lies motionless at the bottom in a spiral form, fair weather; if crept to the top, rain; if restless, wind; if very restless, and without the water, thunder; if in winter at bottom, frost; but, if in the winter it pitches its dwelling on the mouth of the vial, snow.

Signs of Weather from the Barometer.

Barometer. In calm weather, when the air is inclined to rain, the mercury is low; but when tending to fair, it will rise; in very hot weather when falling, it foreshews thunder; if rising in winter, frost; but, if falling in frost, thaw; if rising in a continued frost, snow; if foul weather quickly on its falling, soon over; if fair weather quickly on its rising, soon over; also if rising high in foul weather, and so continuing for two or three days, before

before the foul weather is over, then expect a continuance of fair weather; but, if in fair weather the mercury fall low, and so continue for two or three days, then expect much rain, and probably high winds.

N. B. In an east wind, the mercury always rises and falls lowest before great winds.

XVI.

A New, Cheap and Simple Apparatus for repeating the Hours, and Quarters in Clocks and Watches. By Mr. J. M. ELLIOT. Communicated by the Inventor.

THE great expence of repeating watches in the first purchase, as well as in the subsequent repairs, have induced Mr. Elliot to turn his thoughts towards reducing their construction to a greater degree of simplicity, and consequently to diminish the charges in both these respects.

New and simple repeating motion.

In the mechanism represented in plates VII. and VIII. the parts and their arrangement are such that without the usual apparatus of pinions, pullies, chains, or racks, the watches repeat both the hour and the quarters, viz. the hour first and the quarter afterwards, or the hour or the quarters separately; or either of these first as may be wished, without interruption, and by one hammer. As several of these pieces have been made and disposed of in this country as well as exported, the practicability as well as the cost are both ascertained, which last is about one third of the former charges.

Plates VII. and VIII. represent the parts of this new machinery, Fig. 1. shews the principal pieces as attached to the upper pillar plate by means of a pottance A B C D, called the repeating pottance, kept to its place by the screw x.—a b shews the pendant attached to a steel axis upon which the repeating apparatus is fixed and carried. In the actual time piece these parts are as close to each other as convenience will allow; but they are here represented for the sake of distinctness, as if at a distance from each other. Thus they are made to act and perform the striking by turning the axis of the pendant to the right or left.

c is a part of the bell seen edgewise, d and l are two arms or pallets, which being fitted upon squares on the axis are

New and simple
repeating mo-
tion.

moved round whenever the axis is moved by means of the pendant. The first *d* serves to drive the work for striking the hour, and the latter *l* the work for the quarters. The first of these apparatuses consists of *e* the hour-locking snail (see also *Fig. 2*.) which is fitted on the axis by a round socket or pipe, upon which socket also is fixed (steadily) the hour repeating wheel *g* (*Fig. 1* and *2*.) and (on a round part) the ratchet wheel *h*, which last is drawn by a spiral spring *k k* in the direction opposite to that which is produced by the pallet *d* (*Fig. 1* and *2*) *s* is the hammer urged by its spring *v* outwards, and the designer has, I am sorry to perceive, omitted to make a small tail from the center pin near *k* towards *g* the hour repeating wheel. Now the action of striking is simply this; when by turning the pendant *d* acts upon the small pin in *e*, the wheel *g* with its ratchet wheel *h* are carried round, and the teeth of *g* act on the tail of the hammer, and give more or fewer strokes, according to the number that pass.

The number is regulated thus, *a* (*Fig. 4*.) is the star-wheel as usual, and *b* the hour snail screwed on the same; *c* is the click with its back spring, and *n z* is the hour-locking lever, which terminates in an hook at *z* that may be seen in *Fig. 2* bearing against the hour-locking snail *e*. This hook will evidently be nearer to or farther from the axis of the locking-snail *e*, according to the portion of the other end of the lever which bears against the hour snail *b*, and will consequently stop the pendant in its motion sooner or later by its opposition to one of the stops of the snail *e*. As the star wheel *a* is carried regularly round by the canon pinion as usual, its situation will determine the number of strokes.

The instant the pendant is let go, the spring *k* acts upon the ratchet wheel, and restores the original situation ready for striking again.

After this account of the hours, the striking of the quarter will require little explanation, *l* (*Fig. 1* and *5*.) is the pallet or arm acting (by a reverse motion of the pendant,) on the locking snail *m*, which carries the contrate wheel *p*, for striking the quarters by its action, upon another tail *r* of the hammer and the original situation is restored by a ratchet wheel *O* on its spring. (see *Fig. 6*.)

In *Fig. 4*. is seen the quarter snail *g* with its lever *h k*, whose hook for regulating the number, may be seen at *D* in *Fig. 1* and the snail is prevented from going back by a fixed hook.

SCIENTIFIC NEWS.

Prize for the Artificial Composition of Palladium.

IN consequence of the intimation received by the anonymous person of which a copy is given in the last number of this journal, at page 75, I waited upon Mrs. Forster, who personally assured me that she holds the sum of twenty pounds, (which was paid to her by the same unknown person who placed the palladium in her hands,) under the trust or engagement to pay the same to the bearer of the certificate mentioned in the said paper, at any time before Midsummer next; and that, if not claimed during that time, she shall afterwards consider herself at liberty to repay the money when demanded by the same unknown person.

I have therefore requested Charles Hatchett, Esq. F. R. S. and Edward Howard, Esq. F. R. S. to join myself as judges of the product which may be made in our presence, agreeably to the before-mentioned paper; to which proposal they have assented. And I have no doubt but that the commission will enable me to present to my readers an account of whatever may be the result of this public invitation.

Prize adjudged at the last Public Meeting of the National Institute.*

THE decree of the government, May the 2nd. 1803, that authorized the Institute to accept the capital of 10000 francs, [417£.] offered by Lalande, expresses, "that, agreeably to the intention of the donor, the interest should be laid out annually in a gold medal, which, or its value in money, should be given to the person, in France or elsewhere, the members of the Institute only excepted, who should have made the observation, or written the memoir, that might appear most conducive to the progress of astronomy."

Annual prize for the most useful astronomical paper or observation.

Accordingly, on the report of the committee nominated for the purpose, the physical and mathematical class decreed the prize to Dr. W. Olbers, for having discovered, in the course of the year 10, the planet to which astronomers have given the name of *pallas olberfiana*.

Decreed to Dr. Olbers for the discovery of a planet.

* *La Decade Philosophique*. No. 29. July 1803. p. 142.

The Dr.'s merit
as an astronomer.

Before this discovery Dr. Olbers enjoyed considerable reputation among astronomers, for a dissertation on the most easy and commodious method of calculating the orbit of a comet; and for the laborious calculations and observations he had made, to re-discover Ceres, or Piazzi's planet, which in fact he did perceive, and was the first to announce to us.

*Teylerian Society at Haarlem.**

Teylerian society's prize
question.

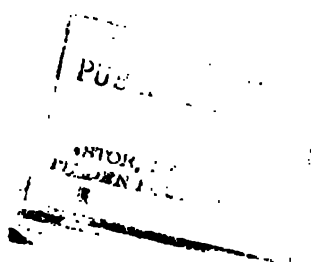
THIS society has proposed the following question:

"Has the application of pretended supernatural principle to physics contributed to the progress of the science? or does not the history of this science prove on the contrary, that all the progress made in natural philosophy has been owing to observations, experiments, the conclusions drawn from them and mathematical calculations and demonstrations?"

The prize is a gold medal of the value of 400 florins (£40) and the answers to the question, written in Dutch, Latin, French, English, or German, must be sent to the society before the 1st of April, 1804.

* * The explanation of the subjects of the queries of R. B. in our last, (page 71.) are deferred for want of room.

* *La Decade Philosophique*, No. 33. August, 1802. p. 382.



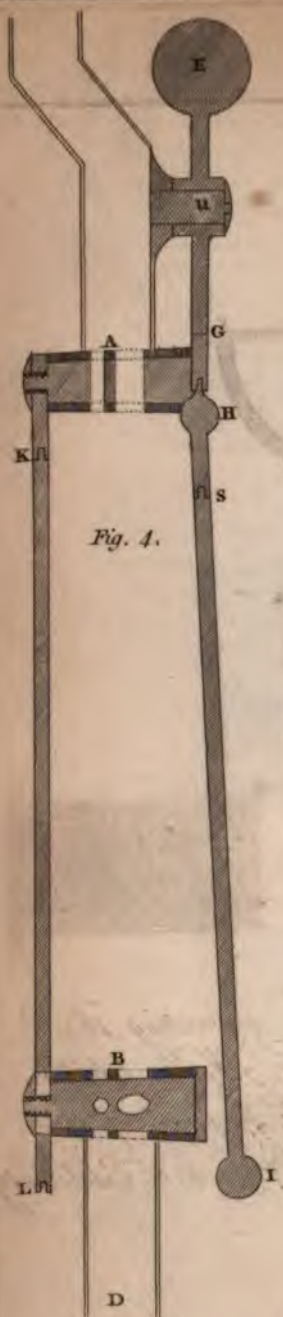
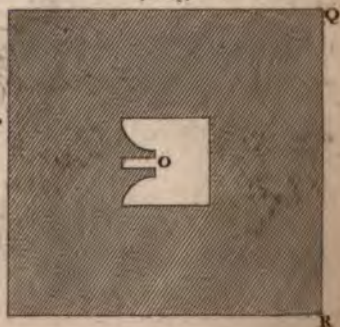
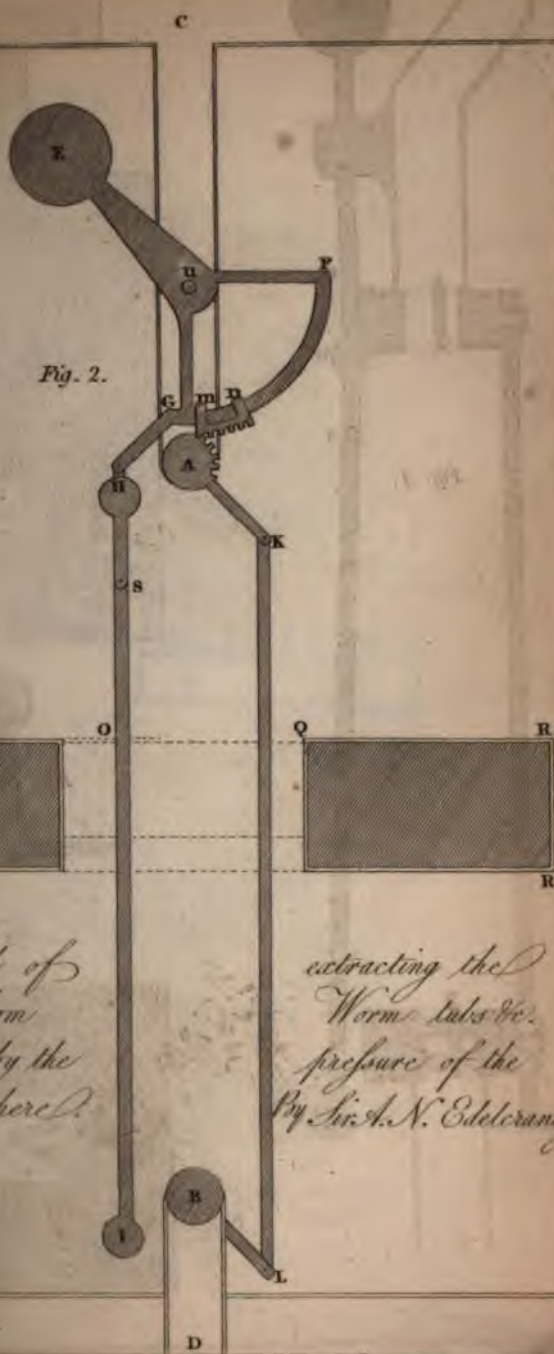


Fig. 3.





Method of
Air from
supplied by the
Atmosphere?

extracting the
Worms &c.
preparation of the
By Sir A. N. Edelcrantz

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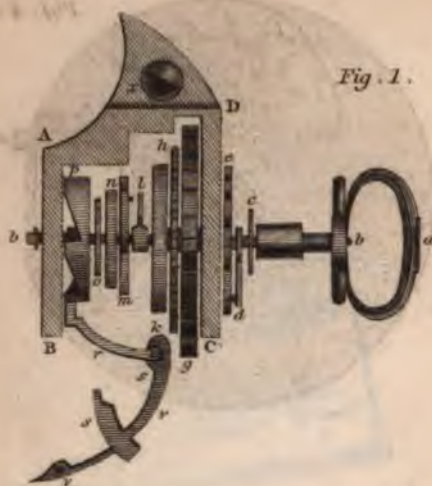
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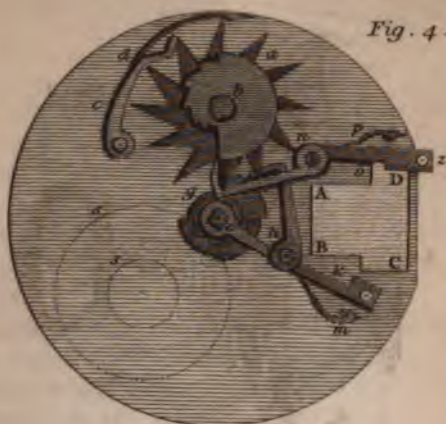
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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MARCH, 1804.

ARTICLE I.

*Description of a new Steam Digester for Philosophical Researches.
By Sir A. N. EDELCRANTZ, Counsellor of the Chancery, and
private Secretary to the King of Sweden; Member of the
Swedish Academy, &c. &c. Communicated by the Author.*

THE interesting and curious phenomena which this machine presented, from its first discovery by Papin, have always rendered it a desirable part of the chemical apparatus; but the difficulties and dangers attending its use, have, at the same time, prevented its applications from being so general, as the importance of the object seemed to promise. Many, and some of them successful, attempts have been made to adapt this instrument to economical and culinary uses; but these being commonly limited within very few degrees of heat above the boiling point, such digesters, though they can be used with safety, are at least inaccurate in the permanence, and uncertain in the determination of the elasticity and heat of the vapours acting in each experiment, which consequently cannot be either precise or comparable with others: and though several ingenious philosophers, such as Watt, Betancourt, Smith of Geissen, Van Marum, &c. have in later times very much improved their scientific application, enough of imperfection still seems to remain to prevent their general use, and to afford sufficient scope for further improvements.

Excellence and
utility of Pa-
pin's digester.

Improvement.

As long ago as the year 1793 I applied my thoughts to render this instrument less imperfect, and though I cannot flatter myself with having succeeded in any considerable degree, a short account of my attempts may not perhaps be thought quite useless.

The usual method of closing the digester is faulty in many respects.

My first object was to obtain a perfect closure for the digester, which is necessary not only for increasing the heat, but also for preserving the contents. In former contrivances this has generally been effected by a metallic cover pressed on the top of the vessel by means of screws, wedges or other mechanical contrivances, with a circular piece of leather, paper, or other soft material between both. I also tried these methods in a great variety of ways, but always found such covers either leaky, on applying moderate pressures, or when the pressure was stronger, too cumbersome for a course of varied and repeated experiments. Besides which, at the degree of 260 or 270° Fahr. the leather, as observed before by Mr. *Beancourt*, is commonly decomposed or burnt, and linen or paper cease to close at a still lower temperature; as soon as the moisture, they are imbibed with becomes converted into vapour. I therefore used another cover of a particular kind, consisting of a thick circular plate, with its edge turned conical, and ground to fit exactly in the under side of a metallic ring of the same conical form, folded to the mouth of the digester, in which the plate was previously included. When the vessel was ready for experiment and the plate lifted up so as to apply to the ring, it closed as exactly as could be desired, and the increased force of the steam, which tends to render other covers defective, had in this a contrary effect. Two difficulties were thus overcome, that of a cumbersome pressing apparatus, and the insufficiency of packings; but as the circular plate in this construction was made to remain constantly in the digester, I found that though it was laid near the side of it, in some experiments this mode was attended with an inconvenience which I was desirous of removing. I succeeded by making another circular ring, turned with both sides conical, to be put between the circular plate and the opening of the digester, fitting exactly to each of them. This ring *ab*, Fig. 5. Plate IX. passes over the cover *cd*, when both are lowered, the ring remaining in the digester; but the circular plate is taken out through the enlarged opening *ef*. Though

New cover which fits by a conical interior surface, without packing.

Inconvenience of this contrivance.

I had

I had by this contrivance,* in a great measure diminished the inconveniences, yet the intermediate ring remaining in the vessel, and the increased difficulty of executing a more complicated instrument, urged me to attempt some other more expeditious mode of closing.

I should almost be ashamed to confess that the most simple and, as I presume, the best contrivance of all, did not occur to me before I had tried the more complicated ones; if many respectable examples had not proved that the easiest and shortest method is often the most difficult to find. The reason is obvious: Simplicity, like truth, is only one; the bye paths of error are innumerable.

Simple and most accurate closure.

This method consists in making the cover simply an oblong, square, or oval plane brass plate, accurately ground against the inferior surface of a similar frame of the same metal, fixed and soldered to the opening of the digester. This frame has an oblong aperture, through which the cover may be put in or taken out at pleasure. When put in and pressed gently against the frame, it will by the means of a little oil,† have adhesion enough to bear its own weight, till the steam begins to act, which will close it perfectly. But for greater security a small piece of wood, fixed with a screw, will keep the plate joined to the frame, and serve at the same time as a handle. *Fig. 1. Plate IX.* represents the longitudinal section of the whole instrument, with all its parts; *ab* is the covering plate, ground and fitted against a square metallic frame *cd*, soldered to the vessel at *ef*, which ought to be made of hammered copper at least $1\frac{1}{2}$ line thick. *Fig. 2* is the transverse section of the same apparatus, and *Fig. 3.* a plan of the brass plate, where the dotted lines mark the opening of the frame *cd*, *Fig. 1.* and the

Description of the same. It is a plate which fits against a surface within.

* The digester with a conical plate, and the safety-piston described below, first executed in Stockholm, 1793, was exhibited to the members of the Royal Society in Berlin, in Feb. 1802; and a description of it, printed in the *Observations Physiques* of Mr. Delametherie, Feb. 1803.

† The different force of adhesion of the same metal, in employing different oils, is remarkable, and may afford a field for future investigation.

‡ Besides soldering, the parts should be joined together with rivets or screws.

part covered by the same. *Fig. 4. hi* is the wooden handle turning round the screw *g*, *Fig. 1, 2.* to connect the metallic planes. It is a kind of button or key.

Peculiar safety valve by which the pressure may be regulated.

My next object was to render the instrument safe, and to place the operator's mind independent of any apprehension of fatal accidents. For this purpose, (besides the strength of the vessel and its different parts, which ought to be as solid as conveniently can be made,) a metallic cylinder *mn* is fixed to the plate *ab*, to the inferior open extremity of which a metallic sphere *mb*, perforated with small holes, adheres by mere friction, allowing the vapour, but not the solid or other matters, to pass into the cylinder, which contains a solid piston *ko*, made of the same metal * and exactly ground into it, so as to sink down by its own weight if lifted, without however permitting the vapour to escape. This vapour when heated to a certain degree, will raise the piston above one or more of the capillary holes *ss*, on the side of the cylinder and then escape. The piston will remain stationary or oscillating at a height, where the quantity of steam escaping is equal to the quantity produced in the same time by that fire, consequently at a low point if the fire is weak, or, if the fire is intense, at the top, where a larger hole presents a compleat issue to the steam and prevents the possibility of accidents.

Conditions for accurate experiment with the digester. 1. To know the temperature. 2. To command the heat, and 3. To render it steady though the fire be not so. The temperature measured by a thermometer.

To adapt the instrument thus constructed to exact and comparative philosophical experiments, three things seem to be required: 1. to ascertain the degree of heat of the interior fluid contents † by a thermometer; 2. to increase that heat at pleasure; and 3. to fix it invariably, if it be desired, independent of the possible increase of the fire, during the course of the experiment.

The first is obtained by means of a small iron cup *p q*, soldered into the plate, and containing some mercury. A fine thermometer passed through a cork, being fixed in the cup, the

* Great care should be taken to make both of the same metal, for in one instance when I had the piston made of the same brass with the cylinder, but of another day's casting, this infinitely small difference was still sufficient to produce a different force of expansion, in consequence whereof the piston, which moved freely at 212°, ceased to move, and was quite fastened at 260°.

† It is perhaps superfluous to observe that a digester is only intended for experiments with fluids or bodies surrounded with them, and consequently never contains materials in a dry state.

degree

degree of heat is easily observed on the stem. This degree will not be exactly the same with that of the interior space; it shews always somewhat less, which fact was long time ago observed by Mr. *Braun* of the academy of Petersburg; but the difference is small, and being once ascertained, by experiment made on purpose, a due correction, by adding that difference, is easily made.

The heat is communicated in my apparatus by a common Argand's lamp, which was always found sufficient and of which the effect may be regulated by the pinion of that instrument. If time be an object, or if the digester is much more than double the size of *Fig. 1.* the scale of which is one third of the real size, two such lamps may be applied. But a lamp with spirit of wine and three wicks will supply fire enough for a temperature of 270 or 280 if the digester be small and exactly closed. As the atmosphere continually takes away a part of the heat communicated to the vessel, it is only the difference of the communicated and dissipated heat which acts on the steam, and determines the limits of its force, when the fire is weak, and the digester of a large size. On the other side, the heat of the interior fluid and the elasticity of steam corresponding to it, cannot, after having lifted the piston, be increased by a continued fire or by a greater number of lamps, but remains from that moment constant, which seems to be a particular property of this construction. A farther increase of heat, as the 2d. desideratum, can only be obtained by opposing a greater resistance to the action of the vapour beneath the surface *k*, which is performed by loading the piston with one or more of the weights *r r*, *Fig. 1.* Thus we may either add new weights progressively, till the thermometer indicates the degree of heat required, or knowing by the former experiments of philosophers the corresponding ratio of increased pressure and heat of elastic fluids, we can load the piston at once with a weight proportional to the intended heat and the surface of its base *k*. This will then rise as soon as that degree is produced, and allowing the steam to escape from one or more of the holes * prevent

The heat given by a lamp.

The heat is constant if the pressing weight be not changed.

What is the second condition.

* By what is said above it is evident that the height of the piston, or the number of the holes open to the steam, is of no consequence, it being always compressed by the same weight, and acting with the same elasticity. The quantity produced is only different.

any

any further increase of the heat, which will remain *invariable* as the third desideratum required.*

Very great disadvantage of imperfect closure of the digester.

Here it seems proper once again to insist upon the great importance of a perfect closure in this instrument. If the heated steam, by inaccuracy in the construction, is permitted to escape, not only a great part of the contents is wasted without any use, before the experiment can be said to begin, which sometimes obliges us to open the digester and fill it a second time, but even some of the materials may be altered and decomposed before the intended effect can take place. Still more, very often in such cases, a strong kitchen fire, with the greatest exertions of the operator, (not to mention the trouble and inconvenience of such an operation) is insufficient to afford the degree of heat, which a single lamp, placed with the apparatus on a small writing table, would produce with ease, and without waste of the materials.

In the present state of natural philosophy, the progress of the science seems to depend in a great measure on the perfection of our instruments. In this point of view the preceding attempts to render a little used but very excellent machine more safe, exact, and convenient †, may, if it should not answer that purpose, at least give rise to other more successful improvements.

* A short description of this contrivance was presented to the *Philosophical Magazine* in June, 1803, and published in December the same year; but by the use of the word *piston* instead of *digester* in a note, an erroneous statement is given in the printed account, which, however trifling it may appear, it is proper to correct, as this part of the instrument has not been changed at all, since it was first made and known in Sweden ten years ago. In the same paper I proposed the use of it for regulating the fire in boilers of steam engines, &c.—and its effect as a *regulator*, both of the *power* of the *steam*, and the *intensity* of the *fire*, depends entirely upon the *weight* being constant and *invariable* in the whole motion of the piston.

† Digesters, as described in this paper, are made by Mr. FIDLER, mathematical instrument maker, 23, Oxford-market, London.

II.

Investigation of certain Theorems relating to the Figure of the Earth. By JOHN PLAYFAIR, F. R. S. Edin. and Professor of Mathematics in the University of Edinburgh.

(Concluded from Page 116.)

21. A REMARK, that is in no danger of being reckoned hypothetical, is, that the conclusion derived from the comparison of degrees of the meridian, with degrees of the circle perpendicular to it, becomes of necessity more liable to error as we advance into higher latitudes. The reason is, that whatever error is committed in determining the magnitude of $D' - D$, must be multiplied into the square of the secant of the latitude, in order to give its full effect in changing the value of the fraction $\frac{c}{a}$. For it has been shewn that $\frac{c}{a} = \frac{1}{2} \left(\frac{D' - D}{D'} \right) \sec^2 \phi$; now, if we suppose the error committed in ascertaining $D' - D$ to be in all cases the same, the error of the fraction $\frac{D' - D}{D'}$ will also be in all cases nearly the same, the denominator D' being but little affected either by the supposed error, or by the change of latitude. But this error, which may thus be considered as a constant quantity, when multiplied into $\frac{1}{2} \sec^2 \phi$, gives the variation or error in $\frac{c}{a}$, which error therefore increases, *ceteris paribus*, as the square of the secant of the latitude, so that, on approaching the pole, it increases without limit, and is ultimately infinite. Comparisons of this kind may therefore be expected to give results the more accurate the nearer they are to the equator, under which circle they will be the most accurate of all. Here, again, however, another circumstance must be taken into consideration, viz. that the method of ascertaining the differences of longitude by the convergency of the meridians, so convenient in surveys of this kind, is applicable only in high latitudes. In a trigonometrical survey, therefore, of a country lying much farther south than Britain, a different method of ascertaining the longitudes of places must necessarily be adopted.

This method by the meridian and its perpendicular is more erroneous the higher the latitude.

22. The

To determine the figure of the earth from measures of the perpendicular in different latitudes.

22. The theorems, which were next proposed to be considered, are those that determine the figure of the earth from the measures of degrees of the curve perpendicular to the meridian, in different latitudes. For this purpose let D' be a degree of one of these curves, in the latitude ϕ' , and D'' a degree of one of them, in another latitude ϕ'' . Then σ being the compression, as before, we have by § 18. $mD' = a + c \sin^2 \phi'$,

$$\text{and also } mD'' = a + c \sin^2 \phi''.$$

Hence $m(D' - D'') = c(\sin^2 \phi' - \sin^2 \phi'')$, and

$$\text{therefore } c = \frac{m(D' - D'')}{\sin^2 \phi' - \sin^2 \phi''}.$$

This formula may be rendered more convenient for calculation, by considering that $\sin^2 \phi' = \frac{1 - \cos 2\phi'}{2}$, so that

$$\sin^2 \phi' - \sin^2 \phi'' = \frac{1 - \cos 2\phi' - 1 + \cos 2\phi''}{2} = \frac{\cos 2\phi'' - \cos 2\phi'}{2}. \text{ But } \cos 2\phi'' - \cos 2\phi' = 2 \sin(\phi' + \phi'') \times \sin(\phi' - \phi''), \text{ wherefore } \sin^2 \phi' - \sin^2 \phi'' = \sin(\phi' + \phi'') \times \sin(\phi' - \phi''), \text{ and } c = \frac{m(D' - D'')}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$$

23. In the same manner, because $mD' = a + c \sin^2 \phi'$, by substituting for c , we have

$$mD' = a + \frac{m(D' - D'') \sin^2 \phi'}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}, \text{ and } a = mD' - \frac{m(D' - D'') \sin^2 \phi'}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$$

$$24. \text{ Lastly, since } mD' = a + c \sin^2 \phi', \text{ and } mD'' = a + c \sin^2 \phi'',$$

dividing the first of these equations by the second, and rejecting the higher powers of c , we have

$$\frac{D'}{D''} = 1 + \frac{c}{a} (\sin^2 \phi' - \sin^2 \phi''), \text{ and therefore,}$$

$$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\sin^2 \phi' - \sin^2 \phi''}. \text{ Hence also}$$

$$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}; \text{ or more conveniently for calculation by logarithms, } \frac{c}{a} = \frac{D' - D''}{D'' \sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$$

25. We

25. We may compare this value of $\frac{c}{a}$ with that obtained in § 18. from other data, in order to determine which of the two methods of finding $\frac{c}{a}$ is to be preferred, under given circumstances. Suppose, for instance, a degree of the curve perpendicular to the meridian, in the latitude ϕ' to be D' , and a degree of the meridian itself in the same latitude to be Δ' ; it is required to find in what other latitude ϕ'' , a degree D'' , perpendicular to the meridian, must be measured, in order that the comparison of D' and D'' , and of D' and Δ , may give values of $\frac{c}{a}$, in which the probable error is the same.

To determine the figure of the earth from measures of the perpendicular in different latitudes,

Here, agreeably to an observation already made, we may, in order to estimate the error produced in $\frac{c}{a}$, in consequence of an error in the determination of D' , and D'' , and Δ , suppose the error to affect $D' - D''$, or $D' - D''$ only, without paying any regard to the variation of D' in the denominator. Therefore, since by § 18 we have $\frac{c}{a} = \frac{D' - \Delta}{2D' \cos^2 \phi'}$, and again by § 24,

$\frac{c}{a} = \frac{D' - D''}{D'' (\sin^2 \phi' - \sin^2 \phi'')}$, if we suppose equal errors in determining $D' - \Delta$, and $D' - D''$, and also that these are the only errors, their effect will be the same, in both cases, if $2 \cos^2 \phi'^2 = \sin^2 \phi' - \sin^2 \phi''$. Now, if we suppose ϕ' the quantity sought, and add $\cos^2 \phi'^2$ to both sides of the preceding equation, then $3 \cos^2 \phi' = \sin^2 \phi' + \cos^2 \phi' - \sin^2 \phi'' = 1 - \sin^2 \phi'' = \cos^2 \phi''$. The latitude ϕ'' therefore must be such, that $\cos \phi'' = \sqrt{3} \times \cos \phi'$. If, therefore, ϕ' be such that $\cos \phi' = \frac{1}{\sqrt{3}}$, the cosine of ϕ'' will be $= 1$, and ϕ'' therefore $= 0$. Now,

$54^\circ 44'$ is the arch of which the cosine $= \frac{1}{\sqrt{3}}$ nearly, therefore

if a degree of the meridian, and of the perpendicular to it, be measured in latitude $54^\circ 44'$, the comparison of these with one another will give a result as accurate as if the degree of the perpendicular, in that latitude, were compared with the degree at the equator, and more accurate of consequence, than if any other degree of the perpendicular to the meridian, were to be compared with D' .

26. Hence,

To determine the figure of the earth from measures of the perpendicular in different latitudes.

26. Hence, also, the comparison of the degree of the meridian, and of the perpendicular to it, in the south of England, is better than if a degree of the perpendicular measured in that latitude were compared with a degree at the equator. For if, in the equation $\cos \Phi'' = (\cos \Phi') \times \sqrt{3}$, we make $\Phi' = 50^\circ 41'$, (or any thing less than $54^\circ 44'$), Φ'' will come out impossible.

27. It may be shewn, too, nearly in the same manner, that if a degree of the perpendicular to the meridian were measured in Siberia, as far north as the latitude of 71° , supposing that to be possible, and compared with a degree in latitude 45° , or even considerably farther south, it would not give a result so exact as the degree of the meridian and perpendicular measured in the south of England. This shews, that the method of ascertaining the figure of the earth, proposed by the authors of the *Trigonometrical Survey*, (*Phil. Trans.* *ibid.* p. 529) as a subject of future inquiry, is less exact than that which is founded on their own observations.

Whether the comparison of a degree of the merid. and perp. in same lat. be more accurate than that of merid. degs. in different lats.

28. We may also ascertain, by the same means, the relative accuracy of the method of finding the figure of the earth, from the comparison of a degree of the meridian with a degree of the perpendicular in the same latitude, and of the method of resolving the same problem by the comparison of two degrees of the meridian in different latitudes.

If, then, D be a degree of the meridian, and D' of the perpendicular, in latitude Φ , and if Δ be a degree of the meridian in a different latitude Φ' , it is required to find whether the most accurate value of $\frac{c}{a}$ will be found, by comparing D and D' , or D and Δ .

Since we have, by what has been already stated, § 4.

$$mD = a - 2c + 3c \sin^2 \Phi, \text{ and}$$

$$m\Delta = a - 2c + 3c \sin^2 \Phi', \text{ we have also}$$

$$\frac{D}{\Delta} = 1 + \frac{3c}{a} (\sin^2 \Phi - \sin^2 \Phi') \text{ and therefore,}$$

$$\frac{c}{a} = \frac{D - \Delta}{3\Delta (\sin^2 \Phi - \sin^2 \Phi')}.$$

Now, it has been already shewn, that, by comparing D and D' we have $\frac{c}{a} = \frac{D' - D}{2D' \cos^2 \Phi}$. Supposing, therefore, equal errors to be committed in the determination of $D - \Delta$, and of D'

$D' - D$, and also paying no regard to the inequality of Δ and D' in the denominators of these fractions, as it is not so great as materially to affect the quantity that is sought for here, we shall have the errors in $\frac{c}{a}$ nearly the same in both formulas, when ϕ and ϕ' are such that $2 \cos^2 \phi = 3 \sin^2 \phi - 3 \sin^2 \phi'$, or when $\frac{2}{3} \cos^2 \phi = \sin^2 \phi - \sin^2 \phi'$, that is, adding $\cos^2 \phi$ to both sides, $\frac{5}{3} \cos^2 \phi = \sin^2 \phi + \cos^2 \phi - \sin^2 \phi'$, and, therefore, $\frac{5}{3} \cos^2 \phi = 1 - \sin^2 \phi' = \cos^2 \phi'$, or $\cos \phi' = (\cos \phi) \sqrt{\frac{5}{3}}$.

29. If, therefore, $\cos \phi = \sqrt{\frac{3}{5}}$, $\cos \phi' = 1$, that is $\phi' = 0$, so that Δ , the second of the degrees of the meridian, must in this case be under the equator. But $\sqrt{\frac{3}{5}}$ is the cosine of $39^\circ 14'$, in which latitude therefore if D and D' be measured, the result, by comparing them with one another, is as exact as if D were compared with the degree under the equator. Hence, if D and D' are measured in a lower latitude than the above, the result will be more exact, than if D were compared with the degree at the equator.

If we suppose D and D' , measured in the south of England, so that $\phi = 50^\circ 41'$; then we will have $\phi' = 35^\circ 7'$, so that D must be compared with a degree of the meridian as far south as $35^\circ 7'$, in order that the result may be as good as when D and D' are compared with one another.

From this it is evident, that the method of comparing degrees of the meridian, and perpendicular in the same latitude, has even an advantage over the comparison of degrees of the meridian in different latitudes, unless these last are taken at a considerable distance from one another.

In this way may many useful conclusions be derived concerning the degree of credit due to measurements already made, as well as with respect to the selection of the places where they are to be made hereafter. On these I shall enter no further at present, and shall only add, that, besides the advantages or disadvantages which the method of comparing together degrees of the meridian and perpendicular in the same latitude has, and which are subjects of calculation, it has another

The former method is preferable unless the degs. of the mer. be far asunder.

The former method also supposes the same observers and the same instruments.

other advantage, which in the case of the British survey is undoubtedly very great, viz. that all the *data* are furnished from one system of trigonometrical operations; executed according to the same plan, with the same instruments, and by the same observers.

To determine the earth's figure by comparing an arch of the mer. with that of a parallel in same lat.

30. One other application of geometrical measurements to discover the figure of the earth yet remains to be considered. This is the comparison of an arch of the meridian with an arch of a parallel of latitude which crosses it. The measure of a parallel of latitude can be executed readily, and is not confined to a small arch, as in the case of a perpendicular to the meridian. The plumb-line, while it is carried along the circumference of a parallel to the equator, tends continually to the same point in the earth's axis, so that there is no difficulty in ascertaining the amplitude of the arch measured, providing there be no unusual disturbance of the direction of gravity. As an arch of a parallel to the equator, however, is not the shortest line between two points on the surface of the spheroid, the measurement along that surface will not give the length of the arch truly. To obviate this difficulty, it is only necessary to follow the method so properly introduced into the *Trigonometrical Survey*, of reducing the measures, both of lines and angles, to the chords and to the planes of the rectilineal triangles contained by them. In this way, the chord of an arch of a parallel of latitude may be determined, however great the arch; and it is worthy of being remarked, that, whatever be the deflections of the plumb-line at the intermediate stations, when the reductions are all properly made, the length of the chord measured will not be affected by them; the amplitude of the arch indeed may be affected by such deflections, if they happen at its extremities; but the effect of this error will be rendered the less, the greater the arch that is measured. We may suppose, therefore, that the chord of a large arch of a parallel of latitude is measured, and the amplitude of the arch itself at the same time accurately ascertained. This last may be done, either by measuring the convergency of the meridians, if it be in a high latitude, or by any other method of ascertaining differences of longitude which admits of great accuracy. The chord being thus given in fathoms, and the arch subtended by it being given in degrees and minutes, the radius of the parallel itself becomes known.

31. Now,

31. Now, if we would compare the radius of a parallel thus found, with a large arch of the meridian, we shall have by that means a determination of the figure of the earth, not less to be relied on than that given in the beginning of this paper. The investigation is easy by help of the theorems in § 5 and 6. Let FO be the radius of a parallel to the equator, which passes through F, the latitude of which is ϕ , and is supposed known; and let FO found by the method just described be $=r$, then,

$$\text{as in § 4. } r = \frac{a^2 \cos \phi}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}} = \frac{a \cos \phi^2}{a \sqrt{1 - \frac{2c}{a} \sin \phi^2}},$$

according to the method of reduction followed in the preceding articles of this paper. Then, because $\sqrt{1 - \frac{2c}{a} \sin \phi^2}$

$$= 1 + \frac{c}{a} \sin \phi^2 \text{ nearly, we have } r = a \cos \phi \left(1 + \frac{c}{a} \sin \phi^2\right) =$$

$$a \cos \phi + c \sin \phi^2 \cos \phi, \text{ or if we divide by } \cos \phi, \frac{r}{\cos \phi} = a +$$

$$c \sin \phi^2. \text{ Let } \frac{r}{\cos \phi} = l, \text{ then } l = a + c \sin \phi^2.$$

32. Again, if ϕ' and ϕ'' are the latitudes of the extremities of an arch of the meridian, the length of which has been measured, and found $=l'$, then, according to § 5 we have

$$l' = a (\phi'' - \phi') - \frac{c}{2} \left((\phi'' - \phi') + \frac{3}{2} (\sin 2\phi'' - \sin 2\phi') \right).$$

If, therefore, m be the coefficient of a , in the former equation, and n the coefficient of c ; and if m' be the coefficient of a , in the latter equation, and n' of c , we have, as in § 6.

$$a = \frac{n'l - nl'}{mn' - m'n}, \text{ and } c = \frac{m'l - ml'}{mn' - m'n}, \text{ or since } m = 1,$$

$$a = \frac{n'l - nl'}{n' - m'n}, \text{ and } c = \frac{m'l - l'}{n - m'n}; \text{ also } \frac{c}{a} = \frac{m'l - l'}{n'l - nl'}.$$

33. In this way of determining a and c , the parallel of latitude may either intersect the arch of the meridian measured or not. If it intersect that arch, this method may have the same advantage that was taken notice of in another solution, viz. that the whole of the *data* may be furnished from the same system of trigonometrical operations. Thus, in the survey of Great Britain, an arch of five or six degrees of a parallel to the equator might be measured, and compared with the whole length

To determine the earth's figure by comparing an arc of the mer. with that of a parallel in same lat.

To determine the earth's figure by comparing an arc of the mer. with that of a parallel in same lat.

length of the meridian, comprehended between the northern and southern extremities of the island, amounting nearly to nine degrees.

It is plain, from what has already been said, that the result deduced from this comparison would possess every advantage, and would be entitled to more credit, than any determination of the figure of the earth that is yet known.

34. On the supposition that, in a survey of a country, the measurement is made along a series of triangular planes, all given in position and magnitude, there is yet another method of determining the figure of the earth, more general than any of the former. On the supposition just mentioned, it is evident, that the length of a straight line or chord, drawn from a given angle of any one of these triangles, to a given angle of any other of them, may be found by trigonometrical calculation. Let the latitudes be observed at the extremities of this chord, and also the difference of longitude; then, from the nature of an ellipsoid, the length of this same chord may be expressed, in terms of the axes a and b , together with the latitudes of the extremities of the chord, and the difference of longitude between them; and this expression being put equal to the length of the chord measured, will give an equation, in which all the quantities are known, except a and b . Further, if $a = b + c$, and if the said expression be reduced into a series, with the powers of c ascending, that series will converge very rapidly, because c is small in respect of a ; then, for a first approximation, we may reject all the terms that involve the powers of c higher than the first, by which means we shall have a simple equation of the form $ma + nc = l$, where m and n are functions of the latitudes and difference of longitude, and l is the length of the chord.

Now, if a similar equation be derived from the measurement of any other chord, these two equations will give a and c in the same manner as in § 6; and thus, from the measurement of any two chords, the figure of the earth will be determined.

35. The length of the chords, thus measured, should be great, so that they may, if possible, subtend angles of several degrees, and their position will be most favourable when one of them is in the plane of the meridian, and the other nearly at right angles to it. The numerical computation will be found less laborious than might be imagined; but the complete

plete solution of the problem, and the full detail of the investigation, I am under the necessity of delaying to some future communication.

To determine the earth's figure, by a general method with triangles.

There seems to be but one difficulty of any consequence that stands in the way of this method of determining the figure of the earth. It arises from this, that the ascertaining the position of the supposed series of triangular planes relatively to one another, involves in it the allowance to be made for the terrestrial refraction, which it must be confessed is not accurately known, and is the more difficult to determine, that it is unavoidably combined with the irregularities in the direction of gravity. It is possible, indeed, to separate these two sources of error, but not without a system of experiments instituted directly for that purpose.

36. The determination of the difference of longitude, which enters necessarily into this problem, except in the case when both chords are in the direction of the meridian, must also be performed with great accuracy. Among the different ways of doing this, that which proceeds by observing the convergency of the meridians, though the best accommodated to the nature of a trigonometrical survey, is not the least liable to objection. For, not to mention that it is only practicable in high latitudes, we must observe, that it always implies a correction on account of the ellipticity of the meridian, which is therefore necessarily hypothetical, and depends on the very thing that is to be found. This inconvenience, however, may be obviated by repeated approximations, and by an accurate solution of *spheroidal* triangles. On this latter subject it was my intention to offer to the Society some theorems, that contain more direct and fuller rules for this kind of trigonometry than any that I have yet met with. I am under the necessity, however, of reserving these, as well as the solution of the problem above-mentioned, for the subjects of some future communication. In the mean time, I think it is material to observe, that the principle laid down by Mr. Dalby, viz. that in a spheroidal triangle, of which the angle at the pole and the two sides are given, the sum of the angles at the base is the same as in a spherical triangle, having the same sides, and the same vertical angle, is not strictly true, unless the eccentricity of the spheroid be infinitely small, or the triangle be very nearly isosceles. The application of the principle may therefore lead into error, unless it be made with due

attention to these restrictions. The gentleman, just named, will forgive a remark, which I certainly should not have made, if I had been less interested for the success of the work, in which he has assisted with so much ability.

III.

Enquiries concerning the Nature of a Metallic Substance lately sold in London as a new Metal, under the Title of Palladium.

By RICHARD CHENEVIX, Esq. F. R. S. and M. R. I. A.*

(Concluded from page 101.)

EXPERIMENTS TO PROVE AFFINITY AMONG THE METALS.

Instances of affinity in metals silver precip. by muriate of platina carried down 21 per. cent. of the metal.

Exp. 1. I dissolved one hundred grains of silver in nitric acid, and precipitated by neutral muriate of platina. The precipitate, well washed and dried, was of a bright straw-colour, and weighed 147 grs. Reduced in a charcoal crucible, it yielded a button weighing 121 grs. and of the specific gravity of 11,6. The difference of weight, between the original hundred grains of silver and the 121, was owing to 21 grains of platina, which had been drawn down in precipitation along with the silver; by an affinity for that metal. This alloy is acted upon by nitric acid, and a great part of the platina is dissolved along with the silver, nor is it very easy to separate them by the common methods.

Mercury and silver precipitated in amalgam by green sulphate.

Exp. 2. I dissolved one hundred grains of silver in nitric acid, and added about 1200 of mercury. I poured the mixed solution into a solution of green sulphate of iron, and obtained a very copious precipitate. When washed and dried, it weighed 939, and was a perfect amalgam, in the due proportion of mutual saturation. Its specific gravity was 13,2; but no mercury remained with it after exposure to heat.

Mercury and gold precipitated by green sulphate gave a fine blue metallic powder.

Exp. 3. I dissolved one hundred grains of gold in nitromuriatic acid, and added to it about 1200 grains of mercury. Green sulphate of iron, poured into this mixed solution, caused a precipitate weighing 874. It was in the form of a fine blue powder, not resembling an amalgam, though wholly metallic. Its specific gravity I could not ascertain; but all the mercury was expelled by heat.

Experiments with recent muriate of tin, and

The re-agents which I used in the following experiments, were recent muriate of tin, and green sulphate of iron. To bring

bring the examples of anomalous precipitations, in mixed solutions of the metals, more clearly into view, it will be necessary to state the action of these salts, upon a solution of each metal when separate.

By recent muriate of tin we have, with a solution of gold, the well known purple of Cassius. With platina, the colour of the liquor is much heightened. With mercury, there is a total reduction. With copper, a reduction from the black oxide at 20 per cent. of oxygen, to the yellow oxide at 11,5 per cent. of oxygen. With arsenic acid, a reduction to the state of white oxide. With silver, with lead, with antimony, no reduction. Green sulphate of iron reduces none of the metallic solutions, except those of gold and silver.

Action of the recent muriate of tin on solutions of gold, platina, mercury, copper, arsenic acid, silver, lead, antimony.

of green sulphate of iron, reduction of gold and silver only.

When mixed solutions of the metals are exposed to the action of recent muriate of tin, or of green sulphate of iron, we have the following results.

Experiments 4, 5, 6, 7, and 8. Muriate of tin, poured into a mixed solution of gold and mercury, precipitates both metals together; and there are no traces of the purple. Mixed solutions of gold and antimony, also of gold and arsenic acid, are acted upon in the same manner. Mixed solutions of gold and copper, also of gold and lead, afford results similar to those of each metal when separate.

Exp. with mixed solutions. Muriate of tin the reagent.

Experiments 9, 10, 11, 12, and 13. With a solution of platina and arsenic acid, muriate of tin gives no precipitate; but the colour of the liquor is more heightened than if the platina had been alone in solution. Platina and antimony give a precipitate by this re-agent, after standing some time; but the effect is retarded by the excess of acid in the solution of antimony. Platina and copper, also platina and lead, are acted upon as the separate solutions of these metals. Platina and silver are precipitated together by green sulphate of iron.

Green sulphate

Experiments 14, 15, 16. Mercury and copper, mercury and lead, also mercury and arsenic, are precipitated in the metallic state by recent muriate of tin.

From these experiments it is evident,

- 1st. That gold has an affinity for mercury, for antimony, and for arsenic.
- 2d. That platina has an affinity for silver, for mercury, and for antimony; and that it is influenced by the presence of arsenic.
- 3d. That silver has an affinity for mercury.
- 4th. That

Deductions of affinity between gold and mercury, antimony, arsenic; platina and silver, mercury, antimony; silver and mercury; mercury and copper, lead & arsenic.

4th. That mercury has an affinity for copper, for lead, and for arsenic.

This series of experiments is not intended as a system of metallic affinities; but as a few facts stated to corroborate an assertion. I am well aware that many others might be noticed; but it is not my intention to enter further into this subject, in the present paper. The general importance of the principle, and the extensive influence it is likely to have upon chemistry, demand that it should be treated by multiplied researches. The experiments that can elucidate it are of the most delicate nature, and require peculiar care; for they do not always succeed, unless performed under the most favourable circumstances.

Mixed solutions of more than two metals are more striking and more complex.

When mixed solutions of three or more metals are exposed to the action of recent muriate of tin, or of green sulphate of iron, their action upon each other appears in a much more striking, as also in a much more complicated point of view.

EXPERIMENTS UPON PLATINA.

Exp. on platina. I shall now state some experiments which I have had occasion to make upon platina, during the foregoing researches. Very little is known concerning this metal, its oxides, or its salts; and, although I have not had occasion to extend the enquiries very far, yet my experiments may serve to establish a few points.

Platina dissolved precip. by lime; redissolved in nitric acid; the acid expelled from the oxide by heat; oxide reduced by simple ignition.

I dissolved a quantity of purified platina * in nitro-muriatic acid, and precipitated by lime. A great portion of platina remained in the liquor, although I had used an excess of the above earth. I redissolved the precipitate in nitric acid, and evaporated to dryness. The result was, a subnitrate of platina. I then exposed the mass, in a crucible, to a heat capable of expelling the acid altogether; and the oxide remained alone. When this was reddened, at a heat which certainly was not capable of melting silver, the oxide was reduced, and appeared with a metallic lustre. The weight of the various

* By purified platina, I have always understood, in this Paper, platina reduced, at a gentle heat, from the salt obtained by pouring a concentrate solution of muriate of ammonia into a concentrate solution of platina.

products

products, in the above experiments, was such as to give the following proportions in the oxide, and the subnitrate of platina.

Yellow oxide of platina is composed of,

Platina	-	-	-	87
Oxygen	-	-	-	13
				100.

Hence component parts of the oxide;

Subnitrate of platina is composed of,

The above oxide of platina	-	-	89
Nitric acid and water	-	-	11
			100.

and of the subnitrate.

But, in the reduction of this oxide of platina, it became of a green colour; and remained during some time in that state. Nitrate of platina sometimes becomes of a pale green at the edges, when evaporated to dryness; and ammonia assumes a green colour when it holds oxide of platina in solution, as we have seen more particularly with palladium. This, therefore, is a second oxide of platina. It contains but seven per cent. of oxygen.

Two oxides of platina; yellow with 13 per. cent. oxygen; green with only 7 per. cent.

I dissolved a known portion of platina in nitro-muriatic acid, and expelled the nitric acid, by pouring in a sufficient quantity of the muriatic; and then evaporated to dryness. By this experiment I learned, that the insoluble muriate of platina is composed of,

Yellow oxide of platina	-	-	70
Muriatic acid and water	-	-	30
			100.

Compon. parts of insoluble muriate of platina;

I then expelled the muriatic acid by the sulphuric, and evaporated again to dryness. I found the insoluble sulphate of platina to be composed of,

Oxide of platina	-	-	54,5
Acid and water	-	-	45,5
			100,0.

and of the insoluble sulphate.

By much the most delicate test for platina is muriate of tin. A solution of the former, so pale as hardly to be distinguished from water, assumes a bright red by a single drop of the recent muriatic solution of the latter metal. If mercury be present, the colour is much darker. Recent muriate of tin, poured into a solution of the muriate formed by the red oxide of mercury,

Muriate of tin is the most delicate test of platina.

converts it into the muriate formed by the less oxygenized acids; but, shortly after, the mercury is reduced to the metallic state. Hence it was, that the alloy of platina and mercury always gave a deeper coloured precipitate than platina, with muriate of tin.

The prussiates do not precip. platina or mercury singly; but they do jointly.

Neither platina nor mercury are precipitated by prussic acid or by the prussiates. But, if sulphate, nitrate, or muriate of platina be poured into prussiate of mercury, an orange-coloured precipitate is immediately formed; and, in some cases, a mixed solution of platina and mercury gives a similar precipitate by prussic acid alone.

Platina is simply precip. by sulph. hydrogen.
Order of affinities of platina with acids.

Platina is one of the metals which are precipitated by sulphuretted hydrogen, without the necessity of a double affinity.

The affinities of platina differ much from what is generally stated in the tables. By the few acids I have had occasion to try, oxide of platina is attracted in the following order: sulphuric, oxalic, muriatic, phosphoric, fluoric, arsenic, tartaric, citric, benzoic, nitric, acetic, and boracic.

Remarkable theoretical observation.

That sulphuric acid should attract the oxide of platina with greater force than the muriatic, is an unanswerable argument to an opinion which was long supported by many philosophers, and which is not yet altogether abandoned by them. Muriatic acid has been said to contribute to the solution of gold or platina, in nitro-muriatic acid, in the same manner as sulphuric acid is supposed to promote the decomposition of water, during the solution of iron by that acid diluted. The affinity of muriatic acid for the oxide of gold or of platina, has been looked upon as the disposing cause that nitric acid is decomposed by those metals. But it is evident that some other action takes place; for, sulphuric acid, which has a stronger affinity for oxide of platina than muriatic acid, does not in the least promote the decomposition of nitric acid by gold, or by platina.

CONCLUSION.

On the nature and use of general hypotheses.

The substance which has been treated of in this paper, must convince us how dangerous it is to form a theory before we are provided with a sufficient number of facts, or to substitute the results of a few observations, for the general laws of nature. If a theory is sometimes useful, as a standard to which we may refer our knowledge, it is at other times prejudicial, by creating

ing an attachment in our minds to preconceived ideas, which have been admitted without inquiring whether from truth or from convenience. We easily correct our judgment as to facts; and the evidence of experiment is equally convincing to all persons. But theories, not admitting of mathematical demonstration, and being but the interpretation of a series of facts, are the creatures of opinion, and are governed by the various impressions made upon every individual. Nature laughs at our speculations; and though from time to time we receive such warnings as should awaken us to a due sense of our limited knowledge, we are presented with an ample compensation, in the extension of our views, and a nearer approach to immutable truth.

The affinities of metals for each other are likely to be of the most extensive influence in chemistry. They will promote scepticism with regard to future discoveries, and throw some doubts upon our present knowledge. Palladium is certainly not less different from the elements that compose it, and from all other metals, than any two can be from each other. Within the last fifteen or twenty years, several new metals and new earths have been made known to the world. The names that support these discoveries are respectable, and the experiments decisive. If we do not give our assent to them, no single proposition in chemistry can for a moment stand. But, whether all these are really simple substances, or compounds not yet resolved into their elements, is what the authors themselves cannot positively assert; nor would it in the least diminish the merit of their observations, if future experiments should prove them to have been mistaken, as to the simplicity of those substances. This remark should not be confined to later discoveries; it may as justly be applied to those earths and metals with which we have been long acquainted.

The affinity of metals for each other, so strongly shewn in the present instance, may induce doubts to the nature of other new metals;

With regard to the metals, we have seen how little dependence is to be placed on specific gravities. A contrary anomaly to that which operates upon platina and mercury, may take place in others; and they may become as much heavier than the mean, as the former become lighter. In this state of union, they may for a long time appear homogeneous, even by the test of chemical re-agents. One of the properties that renders metallic substances so precious is, their easy formation into such instruments as our necessities require. The fragile metals are but of secondary consequence; and, at most, serve to con-

Brittle metals may perhaps be still more simplified.

on those which are ductile, some quality which adapts them better to particular purposes. It often happens that, by being alloyed, two ductile metals become fragile; but we have no instance of the contrary effect in any high degree. It is therefore more to be supposed that we should look to simplification among the fragile metals; and, even at this early period, it may not be too speculative to consider the metallic bodies in an order which may bring together those which possess the greatest number of similar characters.

Comparison of
metallic bodies
with this view;

As an instance of this approximation, it may be observed, that nickel and cobalt strongly participate in the properties of copper and iron. The two former metals were long regarded as mixtures; and the doubts of the ancient chemists, who feared to pronounce as to their nature, may still be proved to have more foundation in truth than the assertion of the moderns, who have declared them to be simple. Acted upon by the same menstrua, forming insoluble compounds with the same acids, and soluble alike in other substances, they have but one or two marked properties that lead us to consider them as distinct metals. But palladium has at least five or six characters, as strong as those of any metal whatsoever, that distinguish it, not only from its elements, but also from all other metals.

and earths also.

Among the earths, this approximation is still more apparent. A leading character of these substances is, their tendency to enter into saline combination, in which they receive new properties, and perform new functions. If we rank them according to this general tendency, we shall have the following order: barytes and strontia; lime and magnesia; glucine and alumina; zircon and silica. And, if we consider them two by two in this order, which is a natural one, we shall bring together precisely those which differ by the smallest number of chemical characters.

Conclusion.

This investigation might be pursued still further; but we must wait the result of experiments: a wide field is open for research. In the dark ages of chemistry, the object was, to rival nature; and the substance which the adepts of those days were busied to create, was universally allowed to be simple. In a more enlightened period, we have extended our enquiries, and multiplied the number of the elements. The last task will be to simplify; and, by a closer observation of nature, to learn from what a small store of primitive materials, all that we behold and wonder at was created.

IV. Observation

IV.

*Observations on some remarkable Strata of Flint in a Chalk-Pit in the Isle of Wight, in a Letter from Sir HENRY CHARLES ENGLEFIELD, Bart. F. R. S. to JOHN LATHAM, M. D. F. R. S. and L. S. **

DEAR SIR,

AS you considered the specimens of flint which I shewed you worthy of the notice of the Linnean Society, I transmit them to you, together with such an account of the situation in which I found them, as may perhaps lead to a guess of the causes of their present very extraordinary condition, and will at least serve as a guide to those who may wish at a future time to inspect the curious pit where I found them. Introductory observations.

Before I enter on the particular description of that spot, I cannot help saying a few words on the lithology of the island in general, which has not that I know of, been described, as it highly deserves, by any naturalist. Had I been equal to such a task, opportunities of observation were wanting, and the phenomenon which I am about to describe, was discovered by me so short a time before I quitted the island, that I had not time to inspect more than one pit, besides that in which I observed it.

The Isle of Wight, which is nearly of a rhomboidal form, lies with respect to its four angles, almost absolutely in the four points of the compass. It is divided into two very nearly equal parts by a range of chalk hills, whose general direction is due east and west. These hills do not, however, lie in a straight line, nor are they at all of equal breadth or height throughout their extent. At Bembridge, where they form the eastern point of the island, they rise abruptly from the sea to the height of about 400 feet; and bending a little to the northward, they continue of nearly the same elevation, and a very narrow breadth, till they terminate at the valley through which the Medina runs. To the west of the Medina the range grows considerably wider and is subdivided into several subordinate vallies. This additional breadth gives the southern limit a great curvature to the south, while the northern line remains nearly straight. Their elevation increases much, and at Mottistoun is 700 feet. The acute and perpendicular Geological account of the Isle of Wight.

* From the Transf. of the Linnean Society, vol. IV.

Geological account of the Isle of Wight.

promontory in which they terminate to the west, well known by the name of the Needles is nearly as high as Mottistoun. Beside the valley of the Medina, this range is singularly interrupted by two vallies exactly similar to each other, at the two ends of the island. Brading Haven renders Yaverland at the east almost an isle, and the Yarmouth inlet cuts off the western end so nearly, that at high tides it is quite insulated at freshwater gate.

To the north of this range of chalk hills the soil is chiefly clay, with a superstratum, in many parts of gravel, the clay is intersected with many beds of stone of different qualities, and which appear to lie in great confusion. Of these some are grit with a slight admixture of calcareous matter, others have nearly equal parts of sand and lime, and others are purely calcareous. In the first which are of great hardness, very few extraneous bodies appear. In the second, are many fine impressions of shells, while the last are almost entirely composed of moulds of turbinated shells, so as to appear quite honey-combed by them. This stone is however, of great durability for the walls of Cowes Castle, which was built by Henry VIII. and is exposed to the sea air from the west and north, are as perfect as on the day in which they were built. Below all these strata of stone at east Cowes, and just above a bed of black and solid clay is a stratum of shells about two feet thick, of which a specimen accompanies this; and which is totally composed of these shells without any admixture or earth whatsoever. As the Lea makes great inroads here, vast heaps of these shells lie on the beach, and seem just washed up by the waves, instead of being torn from their bed in the cliff. They appear nearly in the same state as those on the Hampshire coast, which have long been famous among naturalists. In the bed at east Cowes, there appears, however, no variety; for I could see no species but what are here exhibited.

Whatever confusion in the strata appears to the north of the chalk range, or in that range itself, disappears to the south of it, where the strata are nearly in a horizontal position, and singularly regular and undisturbed. The sea coast from Bembridge, south to the Needles, except in the small extent of Sandown Marsh, is every where higher than the immediately contiguous land of the island, and to the south-east, rises into

a vast range of hills running from Dunnose west to St. Catharines. The substratum of these hills seem every where to be clay lying in strata of different colour and purity. The lowest is black and very hard; approaching to shale. Above this some strata have a great mixture of sand, and take the appearance of a soft stone, breaking into very regular cubical forms. These strata extend over the whole southern part of the island, and terminate against the chalk range very suddenly. Above the clay strata is a bed of stone in thin layers, and of very mingled materials, but in general very hard. Great quantities of chert or flint modules appear in this stone. The general thickness of the stratum is from 150 to 200 feet. Above this the highest hills of the range have a stratum of chalk, not pure or white as that of the chalk range properly so called, nor producing flint so black.

The height of Dunnose is 800 feet above low water mark, St. Catharine's hill is at least 850. Of the former I had no opportunity of examining accurately the thickness of the strata, but at St. Catharine's the strata are as follow,

Chalk	-	250 feet
Stone	-	200 feet, or perhaps not quite so much
Clay and Sand	-	400 feet,

850.

This arrangement accounts entirely for the formation of that singular coast called the Undercliff, which extends from Dunnose to St. Catharine's; and is composed of the confused fragments of the upper stratum of rock, which have given way and rolled down, as the substratum of clay has been washed away by the sea. In most parts the process seems nearly at a stand; the coast being now protected by the fallen rocks; but at St. Catharine's, great devastation is still taking place. The earth-fall mentioned last year, was a very small operation, when compared with the relics of former convulsions.

From this short sketch of the general position of the strata in the island, I return to the particular subject of the present paper.

The chalk pit, which I am about to describe is situated on the northern edge of the chalk range just out of the village of Carisbrook, and about an hundred yards beyond the division of the roads to Yarmouth and Shorwell. The pit is open to the

Geological account of the Isle of Wight,

Description of a chalk pit at Carisbrook, with seams of flint, singularly broken or shattered.

the east. The strata of chalk are very regular from two to five feet in thickness, and divided by seams of flint from six inches to nine inches in depth: The flints are as usual in nodules of different sizes from the size of the fist to twice the size of a man's head. The whole dip northward with an inclination of at least 67 degrees. Perpendicular fissures run through the whole from north to south, the sides of which are nearly as flat and smooth as a wall. As these fissures are followed with convenience in working the pit, an extensive face was laid open when I saw it, and the appearance was as in the annexed sketch. See *Plate XI*. On examining the beds of flint nearly, I was astonished to find that every flint, though lying in its place, and retaining perfectly its original shape, was more or less burst and shattered, some few were only split into large pieces, but the greater part were broken into small fragments, and some absolutely reduced to impalpable powder. From one which had suffered the most, the annexed specimen was taken. The powder was so very fine that I conceived it must have been mixed with chalk; but on washing it with diluted marine acid, I found that it was purely siliceous. Indeed the chalk which surrounds these flints is uncommonly solid, and does not exhibit cracks or marks of any violence except the great fissures before mentioned. A specimen of the flint powder after washing in the acid is sent with the other.

I must observe that I had but an imperfect opportunity of inspecting the flints which lay at a distance from the fissure, such however as I could see in the bed then working appeared to have been less shattered in proportion as they were more remote from the fissure, but all had suffered more or less.

About 200 yards below the pit, and nearer to Carisbrook village, the road is in part cut through the chalk, and the beds of flint exposed by that means exhibit the same appearances as those in the pit above.

Observations of
the flints in
another pit.

The chalk pit above Shide Bridge, which is the only one I had an opportunity of examining after my discovery of the phenomenon above described, presents in some degree the same appearances, but does not afford so good an opportunity of viewing the strata as that at Carisbrook. The strata did not appear to me to lie so regularly nor the flints to be disposed so much in beds as at Carisbrook. They were however ex-
tremely

extremely broken and shattered and apparently the most so where they lay most in strata. The strata had also a great inclination or dip to the north.

Although it would be rash to attempt to account for this very singular state of destruction of the flints in the Carisbrook pit, yet it is impossible not to offer some conjectures on the subject. There can be very little doubt that the strata though now so inclined was originally formed in a horizontal position. When the tremendous convulsion took place which sunk them to the situation in which they now appear (at which time the channel which separates the Isle of Wight from the main land was perhaps formed) the strata of chalk, in the act of subsidence had a tendency to slide on each other, and this would be exerted most sensibly where from the admixture of the flints the cohesion of the parts of the chalk was the weakest. This motion or rather strain of so enormous a weight might in an instant shiver the flints, though their resistance stopped the incipient motion, for the flints though crushed to powder are not displaced, which must have been the case had the beds slipped sensibly. This conjecture is perhaps strengthened by what I observed in a few detached nodules of flint in the chalk strata which did not appear to have suffered as those in the beds of flint have done. I may here add that it seemed as if in some places the fine powder of the flints had run down, and invested the nearer parts of the fissure with a thin coating of the agglutinated dust; but this may possibly have taken place since the face of the fissure has been exposed to the weather.

Conjecture as to the cause; viz. that the strata at the period when their dip took place were inclined to slide on each other, and the flints were crushed by this enormous action.

Perhaps it may not be totally foreign from the present subject to mention that in a very great chalk pit at the village of Preston, a mile north of Brighthelmstone, in which the flints lie in a very regular and nearly horizontal strata, but which has also vast perpendicular fissures in the chalk; the fissures are in many places filled to a considerable extent with a very thin vein of pure flint exactly as if the flint, not being quite hard when the fissures took place, had been squeezed out of the beds and run into the fissures as soft pitch would do. I do not mean at all to say that this was the case, but merely to describe the appearances. In the chalk pit just below the church at Brighthelmstone another singular appearance may be seen. The upper part of the chalk is in separate masses, not properly rubble, but with all their tender angles sharp exactly as if just broken

Appearances of a chalk pit near Brighthelmstone.

broken to pieces to put into the lime kiln, and quite clean, nearly of a size, and almost without any chalk powder mixed with them.

I remain, &c.

Southampton, Jan. 22, 1800.

V.

Description of a mercurial Air-pump, of unlimited exhausting Power, with a Wooden Piston, working in a Wooden Cylinder, &c. By SIR A. N. EDELCRANTZ, &c. &c. Communicated by the Author.

Construction of an air pump in which no residual space is left.

HAVING resided a long time in a place, where no mathematical instrument maker could be had, I was obliged to contrive some method of constructing an air-pump, in which common workmen and common materials could be employed. Besides which, as in almost all former constructions of this instrument, a small space exists in some part of them, where the air remains condensed, and consequently a limit is fixed to the expansion, I was desirous, if possible, to remove this seemingly material, though in good pumps infinitely small defect. These considerations led me many years ago to a contrivance, of which the following short description may give a notion, reserving a more detailed one, with a more particular account of its effects, to a future publication.

Description with a drawing. It operates by mercury raised and lowered in a wooden pipe, by a plug, and the upper communication with the receiver is made through an iron sock.

A B C D, *Plate XII.* is a solid piston of wood, cylindrical or square, * moving up and down in a similar wooden box or tube, E F G H, by means of rack-work at H. The piston fills the box in its whole length as exactly as possible, without touching and producing friction against the sides. For that purpose the piston is guided at the lower end by an iron rod K I, fixed in the bottom of the box and entering the piston, and at its upper part by a roller, which runs along the inner surface of the barrel. The cavity of the wooden box extends through the canal F M † to the iron cock O, and communicates at M with a per-

* I have, in the execution, preferred the square form, as more easy to make exact and equal in its whole length.

† All the wooden parts are made of strong oak, and are covered with a good resinous varnish, to prevent the absorption of mercury pendiculi

pendicular glass tube M N and the glass sphere P. This sphere is connected with an iron cock Q R, having two opposite perforations, one going horizontally to R, where a small outside valve permits a communication with the glass ball S Y; the other, placed perpendicularly, opens a passage from the common receiver V T to the sphere. The instrument being thus constructed, the box H E F G, is filled with mercury to the dotted line *a a*, or about six inches, and the piston being alternately pressed down or raised as usual by the crank work, the effect of the machine is as follows:

Air pump operating by mercury in a wooden barrel, &c.

The cock Q R being turned as in the figure, the communication with the receiver is shut, and that with the external air, through a small hole S in the glass sphere S Y, being open, the piston in descending presses upon the surface *a a*, and forces the mercury out of its place, it consequently rises in the small space between the piston and the wooden tube, *a a* to *b b*, and at the same time in the glass tube N P to a corresponding height N. The pressure being continued, the mercury still rises so as to fill the whole tube, the sphere P, and last of all makes its way through the cock Q R and the valve R, forcing every particle of the air contained in M N P out before it. When a drop of mercury appears in Y, the piston is moved the contrary way, and by its ascension the pressure on the mercury is diminished, which sinks and evacuates the sphere P, leaving a place for the air from the receiver which is admitted by turning the cock Q R, in the other direction *. P being filled with air, the cock is turned again, the piston again descends, and expels the air, by raising the mercury a second time, and in this manner the exhaustion may be continued at pleasure: The mechanism and effect of this instrument being, as I presume, rendered clear by the preceding description, I shall only add a few remarks.

The necessary force to press down the piston, being a little more than the whole weight of a column of mercury of the basis D C, and the height of the barometer, is considerable in the beginning of the operation, but diminishes as in other air-pumps, when the air in the receiver becomes more rarefied; when the external atmosphere acts more powerfully in support

* The alternate turnings of the cock may easily be performed by a simple appendage connected with the motion of the piston.

Air pump operating by mercury in a wooden barrel, &c.

of the column M N, which in every successive stroke becomes higher, till the exhaustion is nearly compleat, when its height will be that of the barometer.

In order to equalize the pressing force more nearly during the whole operation, the upper end of the piston may be loaded with a weight equal to half the original pressure.

The mercury being a fluid, fills every angular space as it passes, till it opens its way through the valve R; consequently no particle of air can be left in a condensed state to be more rarified or diminished by the next stroke of the piston. By this means the common defect of air-pumps seems to be remedied, and the exhaustion unlimited. Besides, as neither moisture or oil are introduced in this pump, the vapours arising from them in great exhaustions are prevented.

The quantity of mercury required, when the diameter of the wooden box and of the sphere P are four inches, will be about sixty pounds, which if taken out by the cock O, may serve also for another useful apparatus in the same laboratory: the mercurial air holder.

Several projects for mercurial air-pumps have been published before, although I think none executed; but a comparison will, as I suppose, easily shew this construction to be quite different from any of them.

For greater solidity, I have in the execution made three-fourths of the tube M P of iron. The sphere P is likewise fixed and fastened by means not shewn in the figure, but which may without difficulty be apprehended.

Excepting the cock Q R, which requires some precision of workmanship, all the parts of the machine may be executed by common workmen, such as carpenters, smiths, &c. who are every where to be found.

VI.

On the Combinations for supplying Worm Tubs with Cold Water, by the Syphon, with an easy Method of securing the Joints of such Hydraulic Apparatus against the Admission of the External Air. In a Letter from Mr. WM. CLOSE.

To Mr. NICHOLSON.

SIR,

Dalton, Feb. 13, 1804.

THE combinations which have lately appeared in your Journal, for supplying worm tubs, condensers, &c. with cold water, have recalled my attention to certain experiments which I formerly made with the syphon; and I am of opinion that the method proposed will succeed very well upon the simple construction of the apparatus delineated in *Plate IV. Vol. VI.* of your Journal, if all the parts are made air-tight, and a sufficient quantity of water be provided to keep the syphon in constant action, under an impending column of two feet. For the air disengaged from the water in the worm-tub will rise into the bend of the syphon at B. *Plate IV. Vol. VI.* and be carried down the descending column and emitted at C.

Reasons why the syphon principle is applicable to the supply of worm tubs, &c.

The worm tub may be effectually secured against the admission of the external air through the joinings of the staves, by being placed in a larger tub filled with water; no air then can possibly enter the interior tub so long as it is covered, and the workmanship must be very bad, if the syphon should draw the water out of the exterior vessel, so fast as to produce any inconvenience in replenishing it as often as it subsides.

Easy method of excluding the external air, by surrounding the worm tub with water.

The worm must be made air-tight before it is fixed in its place. If the supply of cold water at A should be interrupted, the fluid in A will subside to a level with the surface of C, and, if much air be disengaged during this suspension, and while the water is impending in the apparatus, the syphon will probably not resume its operation: In this case the two apertures must be closed, and the apparatus filled with water.

To set the machine at work when stopped.

You will remember the principles on which I planned the construction of a machine for raising water by the syphon. I have the complete model of such a machine: it has four light, self-acting valves, and when these are not prevented from closing, by impurities in the water, &c. it performs very well.

Account of the effect of a syphon engine formerly described.

The

The syphon is not quite two inches wide; it rises about 34 inches above the reservoir in which it is placed, and the descending column is six inches longer than the ascending one. The machine is constructed with all the parts represented in *Plate III. Vol. I.* but under a different arrangement. It delivers about three pints of water every minute, at 31 inches above the reservoir: the alternations are repeated every five seconds.

Its power in carrying down air.

In the operation of this machine, a bulk of air equal to three pints of water is carried down the descending column every minute, when the fall is only six inches; but I am certain from experiment that the syphon would clear itself of a much greater bulk in the same time.

Hence the worm tub syphon will be effectual in carrying off the air extricated from the water.

In an experiment on the 29th of May 1802, with a syphon of the above dimensions, and a fall of six inches, it appeared by a graduated bottle which received the raised water, that the syphon would take a quantity of air equal in bulk to four ounces of water at least, every three seconds. I think, therefore, that the small quantity of air which will be disengaged from the water in the worm tub at a low temperature, by the operation of the syphon, with a fall of two or three feet, will be carried off as fast as it is produced, unless the pipe be very narrow in proportion to the tub,

I am, Sir,

Your's respectfully,

WILLIAM CLOSE.

VII.

An Answer to Dr. WOLLASTON'S Statement, "Of an Improvement in the Form of Spectacle-Glasses." By Mr. WILLIAM JONES, F. Am. P. S. Optician, Holborn.

The spectacle glasses of Dr. Woollaston censured.

OBSERVING in your Journal for last Month, that Dr. W. H. Woollaston, by a paper inserted therein, is attempting to introduce into the construction of spectacles, the well known and obsolete form of lens called a *meniscus*; I beg leave, Sir, to offer through the channel of your scientific and impartial record, a few observations on his arguments; and my reasons why I do not consider the contrivance as entitled to any claims either of novelty or improvement.

When

When a printed book, or other object, is received through a convex spectacle-glass, or other lens of a short focal distance, such as from seven inches down to four inches or less, the indistinctness observed of the surrounding parts, when the central appear distinct, arises from the spherical figure of the lens, and is by Opticians called the longitudinal aberration of the lens. There is another kind of aberration connected with this lens, called the lateral aberration, which is occasioned by the prismatic form of the lens, producing different refrangibility of the rays of light, and blending the prismatic colours with the appearance of the object. It is the longitudinal aberration only that I have now occasion to consider. This aberration in lenses of the same foci, increases with the diameter and thickness; and of the same diameter is in the inverse proportion to the foci.

Aberration of
light refracted
by lenses.

The rays issuing from distant objects, are more parallel to each other when incident upon the lens, than those from proximate ones; therefore the aberration will be less.

Hence, it may be inferred, that when spectacle-glasses have been made larger in diameter than is sufficient to observe through, the angular extent of objects have very properly been somewhat reduced in diameter, so that a person might use them without much inclining the axis of his eyes, or finding it inconvenient or unnatural to move his head a little. For the aberration is thus diminished, and consequently the objection in a great degree removed, except when the glasses are of very short foci.

Contracting the
diameter of
spectacle lenses
approved.

In concave glasses, the aberration or indistinctness is of a similar nature; the defect of these being from the imperfect divergence of the rays, instead of the imperfect convergence in convex glasses.

Spectacle-glasses are now generally made of the double concave, and double convex forms or nearly so; for a little alteration of figure does not affect the general appearance of objects viewed through them. It is in science, as in other cases, that general utility does not always depend on trifling alterations. Spectacles are recorded to have been invented about the year 1300, and from much reading and many years experience in this small, but invaluable article, I really do not know, that during the elapsed time, any optical instrument of any kind whatsoever has undergone more innovation, and

Remarks on
spectacles and
some of the im-
provements
made in them.

attempts at improvement. Of many within my knowledge, I shall only select the following as entitled to any degree of commendation. *Ayscough's* crown glass spectacles. The bisected glasses of *Dr. Franklin*. *Ribright's* double glasses. The visual glasses of that learned Optician *Mr. Martin*. The square convex form by *Stour*. The patent combined glasses of Messrs. Watkins and Smith, injudiciously called achromatic, consisting of a convex lens, combined with a *meniscus* or concave convex lens. In the various mountings of the frames there is a still greater variety.

The original form most approved.



Notwithstanding these contrivances, universal experience has caused the original and simple form of glasses to supersede them; and it affords an indubitable proof, that it is the best and most convenient that can be devised, when clear glass, accurate tools, and good workmanship are used.

Theorem of Huygens considered as universally applicable.

The theorem given by *Huygens*, and demonstrated by many other subsequent writers on optics, proving that a convex lens having its radii of curvature in the proportion of one to six, has less aberration than any other form of lens, when the greatest convexity is towards the object; and the same for the concave lens; must hold true for any use whatsoever, for which such a formed lens may be required.

Eye glasses of spectacles.

It does not appear to have occurred to *Dr. W.* that the eye glasses used to magnify the images formed by the object glasses in telescopes are of the best form, when with the curves of the proportion above mentioned. In the eye pieces of the best achromatic telescopes, they are always applied, and in high powers, the image frequently subtends an angle from the centre of the eye glass of sixty degrees or more. I have never seen any correct dioptrical theorem, that tended to prove that a *meniscus*, singly, or combined, will answer so perfectly the same purpose.

Remarks on lenses; to ascertain the value of *Dr. Wollaston's* patent spectacles.

The ordinary purposes of vision, are very well answered by the common glasses, under an angle as large as eighty or ninety degrees, and the best artists, or draughtsmen allow, that 60° is as much as a fixed position of the eye ought in perspective to embrace, to convey a just representation on the optic nerve.

To persons, the humours of whose eyes are so decayed as to be deficient in their original refractive power, glasses of short foci

foci will, to them, render the extreme parts of objects somewhat confused; but in a much less degree than to persons, with perfect eyes, or undecayed humours.

Remarks on lenses; to ascertain the value of Dr. Wollaston's patent spectacles.

In telescopes and microscopes, the aberration is usually cut off by the insertion of circular apertures or stops; but in spectacles this is not essentially necessary, nor does the want of them or the figure of the glasses, prove that they are constitutionally bad and prejudicial.

The observation of Dr. W. that only a portion of the glass a little larger than the pupil of the eye, is employed at once, is only just inasmuch as it relates to the mind, being intent on a point of an object; but not so in regard to a general view. For the refractive power of the lens does most admirably collect all the infinite number of pencils of rays, or cones, into one concurrence at the pupil of the eye; where they cross or intersect each other; yet, such is the exquisite subtilty of light, that no confusion or irritation takes place. Man is thus blessed by assisted vision, as he is in vitality by the respiration of air.

Dr. W's inferring the form of a meniscus from the shape of a globe, is manifestly erroneous; and in respect to spectacles inappropriate. A glass globe or sphere without any sensible thickness, to an eye exactly placed in its centre, admits all the incident rays to pass throughout it unrefracted. If the eye deviates from the centre, a refraction will take place, and that in proportion to the thickness of the sphere. Rays of incidence pass perfectly unrefracted through truly ground plane, or parallel glass, to an eye before it; and let the axis of the eye, be ever so much inclosed, unless the glass be very thick, the object will still appear perfect, and no refraction of the incident rays will be observed. It is obvious, therefore, the nearer a lens approaches to the figure of a plane, the more perfect it must be.

The figure of a meniscus, which Dr. W. wishes to adopt, is as different from a sphere, as a plane. Its figure is composed of two portions of spheres, of different radii. When with a positive focus it is mathematically demonstrable, that it has entirely the properties of a convex lens, and with a negative focus the properties of a concave one. When the radius of the exterior curve, is less than that of the interior it is a convex sort of lens, and magnifies; but, when the radius of the interior curve is less than that of the other, it is

Remarks on
lenses; to as-
certain the value
of Dr. Wolla-
ston's patent
spectacles.

a concave lens, and diminishes. It has also been demonstrated, that the nearer the form of a meniscus approaches to that of a plano convex or concave, the more perfect it will be, and produce less aberration.

I shall dispense here with the proofs by algebraical and analytical formulæ, as any qualified reader will find them in the optical works of *Huygens, Molineux, Euler, D'Alembert, Smith, Emerson and Martin*; and many others.

The rays of light issuing from a near object to a spectacle glass before the eye, are in diverging pencils or cones, and the meniscus form of glass of any certain positive focus, will refract them towards a state of parallelism into the eye, necessary to produce distinct vision, in decayed sight, precisely in the same manner as a double convex or plano convex glass of the same focus would do. A meniscus with a negative focus, acts no ways different from the double or plano concave glasses; the rays of light being divergent somewhat to counteract the effects of too great a convexity in the humours of the eye of a short sighted person. Perhaps, it is hardly necessary to observe, that imperfect vision in the optical sense, consists in the long sighted eye, in the rays of light not being sufficiently converged by its humours, to meet on the retina of the eye, but falling beyond it; and in a short sighted eye by the rays converging too much, so as to meet before they reach the retina.

Varying the geometrical figure of a lens, does not constitute any new optical principle, for any of the common species of lenses, may be cut into the form of a square, a triangle, an oval, &c. all figuratively various, but consisting only of one optical principle.

The use of the meniscus has been abandoned by Opticians, by its containing in comparison with other lenses, the greatest spherical surface, and consequently producing the greatest aberration. Reducing the curvatures of the meniscus elongates the focus, in the same manner as in other lenses, and therefore reduces the aberration. Hence in spectacle-glasses that are not of short foci, no perceptible difference will be found to persons unacquainted with optical experiments.

There are various practical methods that will point out to persons the aberration of lenses here spoken of, and that the
meniscus

meniscus causes the greatest of any of the other forms of lenses; but, the following, I would recommend as the most easy and illustrative.

Remarks on lenses; to ascertain the value of Dr. Wollaston's patent spectacles.

Take a meniscus lens about the size of a spectacle-glass, and with four inches positive focus, and take also a plano convex or a double convex, of the same diameter and focus, in a room with one lighted candle, at a distance by night, hold the convex glass near to the white wall or wainscot side of the room, between it and the candle; move the lens backwards and forwards till a clear image of the candle is formed, which will be a distinct inverted image of it: do the same with a meniscus, and there will be this difference observed by the meniscus, that incircling the vivid image of the flame, there will be a faint white light, which is the circle of the aberration; and, evidently shews, that it is the worst form of the two for a spectacle-glass, or any other purpose.

Two convex double glasses placed together in one cell, contain less aberration than one glass of the same diameter and focus; and two plano convex-glasses with their convex sides placed together in one cell, give still less aberration. It is loss of light only that can be objected to. They are too weighty to be adopted in spectacles, but in eye pieces of large telescopes for viewing celestial objects, they have been used with great advantage. To engravers, miniature painters, and other artists, they are most useful, as by short foci and large apertures, they give them the most distinct view of a large surface placed before them.

For the satisfaction of an intelligent person, who may be disposed to have an ocular proof of the properties of glasses as herein advanced, I have constructed a frame, containing a double convex, plano convex, a meniscus, two plano convexes, with their convex sides to each other, all of the same diameter, and the foci about four inches, and by which may be seen that the greatest peripheretical indistinctness is with the meniscus glass. This apparatus will be shewn by application at our manufactory in Holborn.

The meniscus as a figure for a spectacle glass I consider very objectionable. To afford a large field of view, its diameter must be considerable; which for a short focus will increase thickness, protuberance outwards, and weight; and, in concave glasses occasion the frames to be made thicker; the glasses will

will be more liable to be scratched and broken than those of the common form, and when the frames are metallic, more likely to increase than diminish that indelible mark made on the nose by the weight of the frame, so frequently complained of by persons who wear spectacles almost constantly. A great deal of superfluous light also passing through the glasses, must be evidently prejudicial; and, it appears to me that the concave figure of the inner side of the meniscus will act as a powerful reflector to condense the rays of light and heat upon the eyes, and ultimately prove thereby of serious injury.

I have in my possession a meniscus spectacle glass, taken from a spectacle frame, which I can prove to have been made many years ago; and, finally, as this form is neither new in principle, or in practice, I am at a loss to know upon what sort of discovery his Majesty's letters patent have been solicited.

I am,

Sir,

Your's, &c.

W. JONES.

VIII.

*An Account of an Occultation of β Nebulae Sagittarii, by the Planet Mars, on the 17th of April 1796, observed by Sir HENRY ENGLEFIELD, Bart. F. R. S. In a Letter addressed to the Rev. NEVIL MASKELYNE, D. D. F. R. S. and Astronomer Royal, **

DEAR SIR,

Occultation of a fixed star by the planet Mars.

I BEG leave to communicate to you an observation, which I had the good fortune to make, of an occultation of a fixed star by the planet Mars; and which, as far as I can learn, was seen by nobody in this country but myself and Lady Buck, at whose house at Petersham I made the observation, and to whom I showed the planet when nearly approaching to conjunction with the star.

These observations are very rare.

Observations of this kind may be ranked among the rarest phenomena in astronomy. Mr. De La Lande records but

* From the Journals of the Royal Institution, No. 16, p. 38:

four

four since the invention of telescopes. Of these, one only is by Mars, and that so long ago as about the year 1630, by Gallendi. The curious and very important conjunction of Mars with psi aquarii, in October 1672, whence Cassini had hoped to deduce, with great accuracy, the parallax of Mars and the Sun, was missed by bad weather at the moment of the occultation. Since that time, I do not know that any observation of this kind has been made.

The night of April 17, 1796, being very clear, I was led by mere accident to direct my telescope to Mars, about two o'clock in the morning; and I saw with equal pleasure and surprise, a star, about three of his diameters to the east of him, and in a line perpendicular to his horns, he being then gibbous. It was evident that an occultation would take place, and I prepared every thing for the observation.

Account of the observation.

The telescope I used is an achromatic of Ramsden, the aperture 2.75, with a single eye-glass, magnifying 100 times. The clock is a very good regulator, with a wooden pendulum, and to be depended on to less than 2 seconds in the 24 hours. The observations for the time were made with an excellent six inch Hadley's sextant of Ramsden's, and an artificial horizon of mercury.

Instruments.

The position of Petersham, as deduced by a survey made by me some years since, from the Royal Observatory at Kew, which was one of the points settled by General Roy in his great survey, is $1^{\circ} 12'$ in time west of the Observatory at Greenwich in longitude; and its latitude by the same survey $51^{\circ} 26' 34''$.

Place of the obs.

To return to the observation. The sky, during the whole time, was perfectly free from clouds, but was remarkably full of vapours, which at times produced a greater undulation in the limb of Mars, than I almost ever saw. The wind was in the east, and probably the columns of warm air blown from London, mixed very irregularly with the atmosphere of Petersham. This state of the air, and the low altitude of Mars, whose southern declination was $23^{\circ} \frac{1}{2}$, rendered it impossible to distinguish with certainty the star, from the protuberances in the waving limb of Mars at the moment of immersion, though the brilliant and silvery light of the star contrasted strongly with the dull red hue of the planet. The observation of the immersion of the star is therefore uncertain, to near a minute of

Collateral circumstances.

of

of time. At the emerfion the ftate of the air was rather more favourable than at the immerfion; and as the ftar emerged from behind the dark limb of Mars at a fenfible diftance from his enlightened edge, this obfervation is much more certain than that of the immerfion; and is probably true, to five, or ten feconds at moft.

Effect of the
atmosphere of
Mars, probably.

The ftar at its firft emerfion, certainly appeared much fainter than it did foon afterwards; but I cannot pretend to decide, that this apparent diminution of its light was owing to any other caufe than its great proximity to the fuperior light of the planet. Yet it was at firft fo faint as to be very doubtfully vifible, and in lefs than a minute was very confiderably brighter. As in this interval of time, its diftance from the planet was not very much increafed, it is probable that the atmosphere of Mars might have been the caufe, at leaft in fome degree, of this appearance.

Occlutation of a
fixed ftar by the
planet Mars.

*Occlutation of β by the Planet Mars, April 17, 1796,
at Peterfham.*

Immerfion of β per clock $15^h \ 4' \ 48''$

Uncertain to nearly one minute.

Emerfion of β per clock $15^h \ 24' \ 28''$

Certain to five, or ten feconds at moft.

The ftar paffed to the north of the centre of Mars. The immerfion was about one third, or at moft two fifths of his femidiameter north of the centre. The emerfion uot quite half the femidiameter north of the centre, by eftimation.

The clock, at the time of the occlutation,

was faft on mean time $0^h \ 4' \ 21\frac{1}{2}''$

The immerfion was therefore at $15^h \ 0' \ 26\frac{1}{2}''$

The emerfion at $15^h \ 20' \ 6\frac{1}{2}''$

Mean time at Peterfham.

And, Greenwich being $1' \ 12''$ in time eaft of Peterfham, the obfervations reduced to the meridian of Greenwich are,

	Mean Time.	Apparent Time.
Immerfion . . .	$15^h \ 1' \ 31\frac{1}{2}''$	$15^h \ 2' \ 28\frac{1}{2}''$
Emerfion . . .	$15^h \ 21' \ 18\frac{1}{2}''$	$15^h \ 22' \ 8\frac{1}{2}''$

Although this obfervation is fubject, from the circumftances ftated before, to an uncertainty of half a minute in time, as to the middle of the occlutation; yet, as the geocentric motion

tion of Mars, was at this time not quite two seconds of a degree in three minutes, his place (that of the star being accurately known), may certainly be determined by this observation within a single second; with a view, therefore, of ascertaining the accuracy of the tables of Mars, given in the last edition of De la Lande's *Astronomy*, the longitude and latitude of Mars were computed with care as follow :

1796, April 17^d 15^h 12' 18" apparent time at Greenwich.

	Geoc.	Long.	Geoc.	Lat.	Auft.
Mars	8°	27° 7' 37"	0°	20' 41" 40"	uncorrected,

for aberration or parallax. And April, 18^d 15^h 12' 18" his place was 8° 27° 23' 2" 0° 22' 58" 18".

His daily motion in longitude, was therefore 0° 15' 25", and in latitude 0° 2' 16" 38" increasing.

His aberration in longitude at the time of observation, was - 3".97, and his horizontal parallax, that of the Sun being taken at 8".72, was 11".712.

The parallax in longitude and latitude, computed by the tables of the Nonagesimal for Greenwich, given in the *Connaissance des Temps* for 1775 is, Par. Long. + 1".772, and Par. Lat. + 11".2.

The apparent place of Mars at the time of the occultation was, therefore, by these tables,

Geoc. Long.	Geoc. Lat. Aufz.
8° 27° 7' 34".8	0° 20' 52".9.

But the place of the star, as settled at Greenwich, was at the time of occultation,

Long.	Lat. Aufz.
8° 27° 6' 19"	0° 20' 41"

And as the centre of the planet passed about two fifths of his semidiameter south of the star, and the apparent diameter of Mars at that time was very nearly fourteen seconds and an half; the difference of latitude between the star and Mars, may be estimated at three seconds, without possible error of above half a second.

The apparent place then of Mars, as found by observation, was,

Geoc. Long.	Geoc. Lat. Aufz.
8° 27° 6' 19"	0° 20' 41"

and the error of the tables is in longitude + 1' 15".8, and in latitude + 12".

Had

Occultation of a
fixed star by the
planet Mars.

Had this observation been made under favourable circumstances, the diameter of Mars might probably have been deduced from it with a great degree of accuracy; although such determination would always have been subject to uncertainty, from the effects of the refraction of his atmosphere, which it is impossible to estimate, but which seems, from some circumstances, to be very considerable.

In the present observation, two other causes of uncertainty exist. The first is, the doubt of the duration of the occultation; the second, the possibility of mistake in the estimation of the distance of the star from the centre of Mars.

The first of these causes cannot induce an error of more than two thirds of a second in the estimate of his diameter. Probably the error is not more than half that quantity.

The probable error arising from the second cause, may amount to about the same quantity as the former. Of this I formed as near a judgment as I could, by drawing Figure 2, *Plate XII.* and placing the path of the star at its utmost limits of distance from the centre by my eye. If the effects of both these causes lie the same way, an error of a second and an half in the determination of the diameter of Mars, may possibly arise; but it is highly probable, that the error is very much less than that quantity, putting the effects of refraction out of the question.

Having, therefore, to the best of my power, compared all the circumstances of the observation, and the effects of the above causes; I consider the equatorial diameter of Mars, as resulting from it, to be at that time $14''.3$; and as the distance of Mars from the Earth was then 0.74456 , the Sun's mean distance being 1 , the diameter of Mars at the mean distance of the Sun, would be $10''.8$. Dr. Herschel, in his last paper on Mars, makes it $9''.8$.

As all the causes of error in my observation, tend rather to diminish than increase the size of the planet, I am surprised that I should have exceeded his estimate; which is, however, less than that of former astronomers.

This, Sir, is all that has occurred to me on the subject of this very uncommon observation.

You will perceive, that I have not, in my estimations of the diameter of Mars, or of the distances of the star from his limb, made any mention of the spheroidal figure of the planet. The differences in the results would be perfectly insensible,

But, if that element had been considered; and as I never have been able to perceive the difference of his diameters, I cannot but suspect it to be less than Dr. Herschel supposes.

Occultation of a fixed star by the planet Mars.

I am,

Dear Sir,

Your obliged and faithful,

May 23, 1797.

H. C. ENGLEFIELD.

P. S. In the *Connaissance des Temps* for the year VI. page 457, and year VII. page 428, are accounts of an observation of this occultation, made at Aubenas, or Viviers (for it is not certain at which of these places) by Mr. Flaugergue, member of the National Institute. It is not clear from these accounts that he actually saw the emerison: at $15^h 58' 25''$, apparent time, he saw the star, distant from the limb of the planet only by an interval equal to the supposed breadth of the dark part of the disc of Mars, and it was extremely faint, but perfectly white. Its position was about 20° to the west of the vertical, and above the horizontal diameter. Mr. Flaugergue supposes that the star passed within a second of the northern limb of Mars, and must have been hidden ten minutes and an half.

As the place of observation was in a lower latitude than London, the observer there must have seen the star pass nearer the centre of Mars than I did, the effect of parallax being less, and the occultation must in consequence have lasted longer: but my observation proves, beyond a doubt, that the star passed much nearer the centre than Mr. Flaugergue supposes, as I lost sight of the star at least nineteen minutes and an half.

IX.

Letter from Mr. EZEKIEL WALKER, containing a considerable Improvement in Time-Pieces by Mr. BARRAUD, with Remarks.

To Mr. NICHOLSON,

SIR,

THE method of determining the longitude by time-keepers, possesses some advantages superior to any other that has yet been proposed. In the first place, this method is attended with

The determination of longitude by a time-keeper is more easy and can be oftener

made than by other methods. with very little trouble; and, secondly, the longitude can be found oftener by a time-keeper than by any other means. I have not met with a more striking proof of this position, than in the voyage of La Perouse round the world in the ship *La Boussole*. Between the 1st of August 1785, and the 8th of September 1787, this celebrated navigator took only 72 lunar observations; but he took 393 observations on the longitude by his time-keeper No. 19. This is an unequivocal proof of the benefits that navigation derives from this mode of finding the longitude.

Remarkable case in proof. But time-keepers are very expensive, and liable to irregularity. It is to be regretted, however, that time-keepers are expensive and liable to stop, or go irregularly. Were it not for these inconveniences, no other method of finding the longitude need be sought after. But a discovery was made by Mr. Barraud, about the close of the last century, on the effects of oil on time-keepers, which not only reduces them in price, but contributes to their performing with greater precision.

Discovery of Mr. Barraud, by which time-keepers are rendered more perfect, and cheaper. Mr. Barraud had frequently communicated to me his improvements in chronometry, but this discovery on the effects of oil, appeared a matter of so much importance, that I wrote to him, requesting his leave to publish it, and the following extract is taken from his obliging answer.

To Mr. WALKER.

DEAR SIR,

London, Jan. 28, 1804.

Letter from Mr. Barraud. I AM much pleased to find that it is your intention to favour the public with your observations on chronometry, and shall derive satisfaction in contributing my mite to so desirable an end: you are therefore welcome to use such information as my experience of facts enables me to furnish.

Jewelling the holes of time-keepers is injurious; The state of my regulator I have already described [in a former letter], from which I infer, that so far from jewelled holes being advantageous in clock-work, they are absolutely injurious. That they are equally so in chronometers, I have had abundant experience, having found, almost without exception, in chronometers coming off a long voyage, the oil in a much worse condition in the jewelled holes (particularly in those where the friction was considerable) than in the brass ones. I have therefore been induced, in every instance, to reject jewelled holes, and introduce those of brass, and the alteration

the oil in such holes being sooner vitiated than in brass holes.

teration has been constantly favourable to the performance of the time-keeper. I should be happy in having a communication with you *viva voce* on this theme, &c. &c.

I am, &c.

P. P. BARRAUD.

That Mr. Barraud has not made this alteration in his chronometers, in a hasty manner, will appear from the following extract taken from another of his letters.

To Mr. WALKER.

DEAR SIR,

London, July 17, 1800.

SINCE we parted, I have found additional reasons to believe that jewelled holes (where friction is great) are injurious. A box time-keeper which I have recently taken to pieces, on its return from a long voyage, had the oil in the brass holes in a much purer state than in the jewelled ones. In the former it still remained in a state favourable for action, but in the latter, the pivots were so fixed by the tenacity of the oil, as to require force to extricate them; the steel was also deeply stained, and had parted with all its lustre, &c. &c.

Instance of a time-keeper, in which the oil was much more vitiated in the jewelled than in the brass holes.

I am, &c.

P. P. BARRAUD.

From these and many other observations made by Mr. Barraud upon the effects of oil on time-keepers, it appears that small particles of steel are worn off by friction in the jewelled holes, and mixing with the oil, form a glutinous substance that causes the time-keeper to go irregularly.

Most probably by abrasion of the steel pivots.

On the Action of Cold on Oils.

IT is a known property of some oils, that they freeze much sooner than water. The oil of olives freezes at $42\frac{1}{2}^{\circ}$ on Fahrenheit's scale, consequently the purest oil of this kind will lose its fluidity sooner than that which contains some aqueous particles. Hence we may conclude, that watch-makers ought to make choice of that oil which freezes with the least degree of cold; and as cold has no power to decompose olive oil, it need not be rejected on account of its having assumed the concrete form.

Olive oil freezes at $42\frac{1}{2}$ degrees.

In

In consequence of this oil freezing much sooner than water, the following queries seem to claim our attention :

Hence it may perhaps be necessary to adjust and keep time-keepers at temperatures between 43° and 100° .

Query 1. Will a time-keeper go at the same rate when the oil is frozen, that it did when the oil was in a fluid state ?

If this query be answered in the negative, *Query 2.* Would it not then be improper to adjust the compensation balance in frosty weather ?

Query 3. Would it not be better to adjust the compensation for the effects of heat and cold at 43° for the greatest degree of cold, and at 96 or 100° for the other extreme ?

A chronometer adjusted in this manner, should never be exposed to a greater degree of cold than 43 . This may be easily done with pocket chronometers, but to keep the oil from freezing in box time-keepers, in cold climates, more care may be requisite.

Oil has other properties which ought to be carefully examined before it be applied to time-keepers, but this is an inquiry which must be left until some future opportunity.

I am, Sir,

With much respect,

Your humble servant,

E. WALKER.

X.

Letter from C. WILKINSON, Esq. containing Facts upon which Deductions are made to shew the Law of Galvanism in burning the Metals, according to the Disposition of equal Surfaces of charged Metallic Plate.

To Mr. NICHOLSON,

SIR,

IF the following observations should be deemed worthy of insertion in your valuable Journal, I shall take the liberty of troubling you with some further remarks hereafter.

I am, Sir,

Yours respectfully,

Soho Square.

C. WILKINSON.

When the French philosophers had ascertained that a series of galvanic plates produced effects on animal substances in proportion to the number of plates employed, without any regard to the surface of each plate, they concluded from their experiments, that the effects of a galvanic battery on metallic substances are in proportion to the surfaces of the plates employed. I have lately been engaged in experiments with the most extensive galvanic apparatus hitherto constructed, from which some circumstances have occurred, in some respects militating with the deductions of the French philosophers.

A galvanic trough containing one hundred square plates of four inches in the side, each plate formed of a plate of zinc and copper folded together; when charged with a solution of nitrous acid and water in the proportion of about 25 parts water to one of acid, exhibited a power capable of igniting half an inch of steel wire of about one seventieth of an inch in diameter.

When two such troughs were combined endwise, the power was doubled, and when four were thus arranged, the quantity of wire ignited was quadrupled: hence I ascertained in a very extensive arrangement, that the power is invariably in proportion to the number of plates employed.

A galvanic trough consisting of fifty square plates of eight inches each in the side was charged with a similar prepared solution, and this arrangement I found capable of igniting sixteen inches of the same wire as was employed in the former experiment.

When two, three, and four troughs of the same size were combined, the lengths of wire ignited proved to be in proportion to the number of plates employed; so that two hundred eight inch plates ignited more than five feet of wire.

These experiments prove that the powers increase in a greater ratio than as the surfaces of the plates employed. For four hundred plates of four inches, contain an equal surface with one hundred plates of eight inches; and the former will only ignite two inches of the same wire, of which the latter will ignite thirty-two inches.

If this proportion should be observed in experiments with plates of different size, it will appear that the powers of igniting, as measured by the length of wire, increase, in batteries of the same total surface, as the squares of the surfaces of the elementary plates, singly taken in each.

A plate

Introduction.

One hundred galvanic plates of four inches square burned half an inch of wire.

Other troughs added endwise burned more wire in proportion to the number of plates.

A trough of larger plates had much more power,

—which increased proportionally to the number.

Examination of the law of increase in the power of larger plates.

By equal total surfaces the wire burned is as the square of the elementary surface.

Reasoning on the facts. Large plates throw our electricity more rapidly.

A plate of eight inches diameter exposes a surface four times greater than a plate of four inches, and supposing the quantities of electricity given out, to be in proportion to the surface exposed to the chemical action of the fluids, the intensity of (or rapidity of evolution from) each plate of an eight inch trough may be estimated as four times greater than the intensity of electricity from the four inch plates; but the power of ignition is sixteen times greater.

Upon these remarkable circumstances I shall venture to submit to you a few conjectures.

The electricity is thrown out by the parts under the process of oxidation,

A simple galvanic combination resembles in its properties a Leyden phial, that side of the metallic plate which undergoes the greatest change resembles the positive side of the jar, and the other side, the negative, there being no other difference between electricity and galvanism, than the mode in which they are produced. Electricity which appears from the excitation of glass, is occasioned by a temporary change in the capacity of the glass for electricity, while under the rubbing action, being momentarily increased; while galvanism is in some respects the reverse, being occasioned by the diminished capacity of good conducting bodies, when exposed to certain chemical changes. Thus metals which are excellent conductors, when oxydated become non-conductors. While they undergo this change a portion of the combined electricity is evolved, and consequently in the part thus changing positive signs will be evinced.

—and thus by electrophoric action produces a negative state on the unoxidized surface.

The electricity existing in an unchanged metal being perfectly quiescent, is subject, when disturbed, to those laws to which all material substances are amenable; namely of moving in the direction where it meets with the least resistance. As the direction of the electricity is from the substance of the metal towards the surface undergoing the chemical change, it must follow that the other side of the metallic plate which is defended from the action of the fluid will evince negative signs, while the side acted upon positive, nearly upon the same principles as those of the electrified.

The discharge is by a succession of correspondent pairs of plates,

In effecting a discharge of the galvanic battery, the circuit must be completed between the zinc and copper ends of the battery. The two plates nearest the points of contact are first concluded; thus the two next, and so on in successive parts, each plate at equal distances from the points of contact; and

the last pair which is unloaded, are the two plates in the centre of the trough. In an arrangement of only 50 or 60 plates, the rapidity of each successive discharge appears to be so great as not to admit of the intervals being distinguished by our sense of feeling; but when the number is very considerable the sensation produced is jarring and tremulous, such as we may conceive would arise from the action of a constant current of a subtle fluid.

When we expose metallic substances to the galvanic action, the effects produced are greater from a large surface diffused over a small number of plates, than when spread over a greater number of plates; if we suppose 800 particles of electricity given out from a plate of eight inches in diameter, there would only be 200 particles from a plate of four inches (supposing the quantity in the ratio of the surfaces exposed) if two plates be unloaded at once, we shall have from one arrangement 1600 particles of electricity determined in one mass, and from the other only 400; from the former we have a rapid communication of a quantity of electricity of four times the intensity of the latter. And as metallic substances nearly transmit the whole, the effects produced are found to be as the squares of the intensities.

I am now constructing a battery of fifty plates, each plate of which will be two feet in diameter. If the above circle should hold good, the extent of wire that will be ignited will be very considerable. A battery of 50 plates, each plate eight inches in diameter, ignites 16 inches of steel wire of one seven-tieth of an inch in diameter. And as the plates I am now preparing will expose a surface nine times greater, I shall expect that $16 \times 9 \times 9 = 1296$ inches, or 108 feet of wire will be ignited at every contact.

—consequently the whole discharge of equal surfaces is more rapidly made the fewer the plates,

quantity of electricity actually passing in the wire at any instant of time is greater.

A battery now in hand expected to explode above 100 feet of wire,

XI.

Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double-stars; with an Investigation of the Cause to which they are owing. By WILLIAM HERSCHEL, LL. D. F. R. S. From the Philosophical Transactions for 1803.

Read June 9, 1803.

General observations on the theory of the heavenly bodies.

IN the Remarks on the Construction of the Heavens, contained in my last Paper on this subject,* I have divided the various objects which astronomy has hitherto brought to our view, into twelve classes. The first comprehends insulated stars.

As the solar system presents us with all the particulars that may be known, respecting the arrangement of the various subordinate celestial bodies that are under the influence of stars which I have called insulated, such as planets and satellites, asteroids and comets, I shall here say but little on that subject. It will, however, not be amiss to remark, that the late addition of two new celestial bodies, has undoubtedly enlarged our knowledge of the construction of the system of insulated stars. Whatever may be the nature of these two new bodies, we know that they move in regular elliptical orbits round the sun. It is not in the least material whether we call them asteroids, as I have proposed; or planetoids, as an eminent astronomer, in a letter to me, suggested; or whether we admit them at once into the class of our old seven large planets. In the latter case, however, we must recollect, that if we would speak with precision, they should be called very small, and exzodical; for, the great inclination of the orbit of one of them to the ecliptic, amounting to 35 degrees, is certainly remarkable. That of the other is also considerable; its latitude, the last time I saw it, being more than 15 degrees north. These circumstances, added to their smallness, show that there exists a greater variety of arrangement and size among the bodies which our sun holds in subordination, than we had formerly been acquainted with, and extend our knowledge of the construction of the solar, or insulated sidereal system. It will not be required that I should add any

* See Phil. Transf. for 1802, p. 477, and our Journal, V. 72, thing

thing farther on the subject of this first article of my classification; I may therefore immediately go to the second, which treats of binary fidereal systems, or real double stars.

General observations on the theory of the heavenly bodies.

We have already shewn the possibility that two stars, whatsoever be their relative magnitudes, may revolve, either in circles or ellipses, round their common centre of gravity; and that, among the multitude of the stars of the heavens, there should be many sufficiently near each other to occasion this mutual revolution, must also appear highly probable. But neither of these considerations can be admitted in proof of the actual existence of such binary combinations. I shall therefore now proceed to give an account of a series of observations on double stars, comprehending a period of about 25 years, which, if I am not mistaken, will go to prove, that many of them are not merely double in appearance, but must be allowed to be real binary combinations of two stars, intimately held together by the bond of mutual attraction.

It will be necessary to enter into a certain theory, by which these observations ought to be examined, that we may find to what cause we should attribute such changes in the position, or distance, of double stars, as will be reported; and, in order to make the required principles very clear, I shall give them in a few short and numbered sentences, that they may be referred to hereafter.

In *Plate. X. Fig 1.* let us call the place of the sun, which may also be taken for that of the observer, O. In the centre of an orbit or plane N F S P is α Geminorum; and, if any other star is to be examined, we have only to exchange the letter α for that by which such double star is known. This letter is always understood to represent the largest of the two stars which make up the double star; and a general expression for its smaller companion will be x . N, F, S, P, represent the positions of the different parts of the heavens, with respect to α , north, following, south, and preceding; and the small letters n, f, s, p , stand for the same directions with respect to O. $x \alpha P$, is the angle of position of the two stars x and α , with the parallel F P.

Theory of the motions of the stars.

As the motion of an observer affects the relative situation of objects, we have three bodies to consider, in our investigation of the cause of the changes which will be pointed out; the sun, the large star, and the small star, or, as we have shortly called

Theory of the
motions of the
stars.

them, O, α , x . This admits of three cases: a motion of one of the three bodies; another, of two; and a third, of all the three bodies together. We shall now point out the consequences that will arise in each of the cases.

Single Motions.

No. 1. Motion of x . When α and O are at rest, the motion of x may be assumed, so as perfectly to explain any change of the distance of the two stars, and of their angle of position.

No. 2. Motion of α . When x and O are at rest, and α has a motion, either towards P, N, F, or S, then the effect of it, whatever may be the angle P α O, will be had by entering the following Table, with the direction of the given motion.

Motion.	Distance.	Angle.	Quadrants.
α P	— +	+ —	1st and 4th 2 — 3
α F	+ —	— +	1 — 4 2 — 3
α N	— +	— +	1 — 2 3 — 4
α S	+ —	+ —	1 — 2 3 — 4

No. 3. Motion of O. 1st case. When α and x are at rest, and the angle P α O is 90 degrees, a proper motion of O, towards either p, f, n , or s , which will be extremely small when compared with the distance of O from α , can have no effect on the apparent distance, or angle of position, of the two stars; and therefore no other motion, composed of the directions we have mentioned, will induce a change in the comparative situation of α and x .

2d case. When the plane P N F S is oblique to the ray α O, and the angle P α O more than 90 degrees, the effect of the motion of O will be had by the following Table.

Motion

Theory of the
motions of the
stars.

Motion.	Distance.	Angle.	Quadrants.
Op	+	—	1st and 2d 3 — 4
Of	—	+	1 — 2 3 — 4
On	+	+	1 — 3 2 — 4
Os	—	—	1 — 3 2 — 4

3d case. When the angle $P \propto O$ is less than 90 degrees, the following Table must be used.

Motion.	Distance.	Angle.	Quadrants.
Op	—	+	1st and 2d 3 — 4
Of	+	—	1 — 2 3 — 4
On	—	—	1 — 3 2 — 4
Os	+	+	1 — 3 2 — 4

Double Motions.

No. 4. If we admit different motions in two of our three bodies, and if the ratio of the velocities, the directions of the motions, and the ratio of the distances of the bodies be given quantities, a supposition in which we admit their concurrence, may explain the phenomena of a double star, but can never be probable

Motions of the three Bodies.

No. 5. If we admit different motions in every one of the three bodies, $O \propto, x$, and if the velocities and directions of the motions,

Theory of the
motions of the
stars.

motions, as well as the relative distances of the three bodies are determined, an hypothesis which admits the existence of such motions and situations, may resolve the phenomena of a double star, but cannot have any pretension to probability.

The compass of this Paper will not allow me to give the observations of my double stars at full length; I shall therefore, in the examination of every one of them, only state those particulars which will be required for the purpose of investigating the cause of the changes that have taken place, either in the distance, or angle of position, of the two stars of which the double star is composed.

As the arguments in the case of most of these stars will be nearly the same, it may be expected, that the first two or three which are to be examined will take up a considerable space, and the number of double stars, in which I have already ascertained a change, amounting to more than fifty, it will not be possible to give them all in one paper; I shall therefore confine the present one to a moderate length, and leave it open for a continuation at a future opportunity.

α Geminorum.

Observations and
inferences re-
specting the
changes of re-
lative situation
in stars extreme-
ly near each
other.

From my earliest observations on the distance of the two stars which make up the double star in the head of Castor, given in the first of my catalogues of double stars, we find, that about 23 years and a half ago, they were nearly two diameters of the large star asunder. These observations have been regularly continued, from the year 1778 to the present time, and no alteration in the distance has been perceived: the stars are now still nearly two diameters of the large one asunder.

It will be necessary to enter a little into the practicability of ascertaining distances by a method of estimation apparently so little capable of precision. From a number of observations and experiments I have made on the subject, it is certain that the apparent diameter of a star, in a reflecting telescope, depends chiefly upon the four following circumstances: the aperture of the mirror with respect to its focal length; the distinctness of the mirror; the magnifying power; and the state of the atmosphere at the time of observation. By a contraction of the aperture we can increase the apparent diameter of a star, so as to make it resemble a small planetary disk. If distinctness should be wanting, it is evident that the image of objects will not be sharp
and

and well defined, and that they will consequently appear larger than they ought. The effect of magnifying power is, to occasion a relative increase of the vacancy between two stars that are very near each other; but the ratio of the increase of the distance is not proportional to that of the power, and sooner or later comes to a maximum. The state of the atmosphere is perhaps the most material of the four conditions, as we have it not in our power to alter it. The effects of moisture, damp air, and haziness, (which have been related in a paper where the causes that often prevent the proper action of mirrors were discussed,) show the reason why the apparent distance of a double star should be affected by a change in the atmosphere. The alteration in the diameter of Arcturus, extending from the first to the last of the ten images of that star, in the plate accompanying the above-mentioned paper,* shows a sufficient cause for an increase of the distance of two stars, by a contraction of their apparent disks. A skilful observer, however, will soon know what state of the air is most proper for estimations of this kind. I have occasionally seen the two stars of Castor, from $1\frac{1}{2}$ to 2 and $2\frac{1}{2}$ diameters asunder; but, in a regular settled temperature and clear air, their distance was always the same. The other three causes which affect these estimations, are at our own disposal; an instance of this will be seen in the following trial. I took ten different mirrors of seven feet focal length, each having an aperture of 6,3 inches, and being charged with an eyeglass which gave the telescope a magnifying power of 460. With these mirrors, one after another, the same evening I viewed the two stars of our double star; and the result was, that with every one of them, the stars were precisely at an equal distance from each other. These mirrors were all sufficiently good to show minute double stars well; and such a trial will consequently furnish us with a proper criterion, by which we may ascertain the goodness of our telescope, and the clearness of the atmosphere required for these observations. To those who have not been long in the habit of observing double stars, it will be necessary to mention, that, when first seen, they will appear nearer together than after a certain time; nor is it so soon as might be expected, that we see them at their greatest

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

* See Phil. Trans. for 1803, page 232, Plate III.

distance,

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

distance. I have known it to take up two or three months, before the eye was sufficiently acquainted with the object, to judge with the requisite precision.

Whatever may be the difficulties or uncertainties, attending the method of determining the distance of two close stars by an estimation of the apparent diameter, it must however be confessed, that we have no other way of obtaining the same end with so much precision. Our present instance of α Geminorum will show the degree of accuracy of which such estimations are capable, and at the same time prove, that the purpose for which I shall use the estimated interval between the two stars will be sufficiently answered. By an observation of the 10th of May 1781, we have the diameter of the largest of the two stars to that of the smallest as 6 to 5; and according to several measures I have taken with the micrometer, we may admit their distance, diameters included, to be five seconds. Then, as the vacancy between the two stars is nearly, but not quite two diameters of the large one, I shall value it at $1\frac{1}{2}$. From this we calculate, that the diameter of the large star, under the circumstances of our estimation, is nearly $1'',35$: so that an error of one quarter of such a diameter, which is the most we can admit will not exceed $0'',34$. Nor is it of much consequence, if the measure of $5''$ should not be extremely correct; as a small mistake in that quantity will not materially affect the error of estimation by the diameter, which, from what has been said, if the measure was faulty to a second, would not amount to more than one-fifteenth part of it.

Having thus ascertained that no perceptible change in the distance of the stars has taken place, we are now to examine the angle of position. In the year 1779, it was $32^\circ 47'$ north preceding; and, by a mean of the three last measures I have taken, it is now only $10^\circ 53'$. In the space of about 23 years and a half, therefore, the angle of position has manifestly undergone a diminution, of no less than $21^\circ 54'$; and, that this change has been brought on by a regular and gradual decrease of the angle, will be seen when the rest of the measures come to be examined.

The accuracy of the micrometer which has been used, when the angles of position were taken, being of the utmost importance, it becomes necessary to ascertain how far it will be safe to rely on the result of the measures. It might be easily shown

shown that, in the day time, a given angle, delineated on a card, and stuck up at a convenient distance, may be full as accurately measured by a telescope furnished with this micrometer, as it can be done by any known method, when the card is laid on a table before us; but this would not answer my purpose. For, objects in motion, like the stars, especially when at a distance from the pole, cannot be measured with such steadiness as those which are near us, and at rest. The method of illuminating the wires, and other circumstances, will likewise affect the accuracy of the angles that are measured, especially when the distance of the stars is very small. I shall therefore have recourse to astronomical observations, in order to see what the micrometer has actually done.

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January 22, 1802. The position of A Orionis was taken. 1st measure, $52^{\circ} 38'$ south preceding; 2d measure, $54^{\circ} 14'$. Mean of the two measures, $53^{\circ} 26'$. Deviation of the measures from the mean, $48'$.

March 4, 1802. η Monocerotis. 1st measure, $28^{\circ} 18'$ south following; 2d measure, $26^{\circ} 49'$. Mean of the two, $27^{\circ} 34'$. Deviation from the mean, $45'$.

February 9, 1803. α Geminorum. 1st measure, $6^{\circ} 11'$ north preceding; 2d measure, $4^{\circ} 48'$. Mean of the two, $5^{\circ} 29'$. Deviation from the mean, $41'$.

September 6, 1802. γ Coronæ. 1st measure, $89^{\circ} 42'$ north following; 2d measure, $89^{\circ} 38'$. Mean of the two, $89^{\circ} 40'$. Deviation from the mean, $2'$.

When these observations are considered, we shall not err much if we admit that, in favourable circumstances, and with proper care, the micrometer, by a mean of two measures, will give the position of a double star true to nearly one degree; but, as the opportunities of taking very accurate measures are scarce, it will be necessary to have recourse to some more discordant observations.

February 18, 1803. β Orionis. 1st measure, $72^{\circ} 58'$ south preceding; 2d measure, $67^{\circ} 24'$. Mean of the two, $70^{\circ} 11'$. Deviation from the mean, $2^{\circ} 47'$.

But a memorandum to the observation says, that the evening was not favourable. We may therefore admit, that in the worst circumstances which can be judged proper for measuring at all, an error in the angle of position by two measures will not amount to three degrees.

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It will be remarked, when we come to compare single measures which have been taken on different nights, that they are somewhat more discordant; but I have not ventured to reject them on that account, except in cases where it was pretty evident that some mistake in reading off, or other accident to which all astronomical observations are liable, was to be apprehended. Nor can such disagreements materially affect the conclusions I have drawn, when it appears that the deviations happen sometimes to be on one side, and sometimes on the other side, of the true angle of position. For, since that angle is not a thing that will change in the course of a few nights, the excess of one measure will serve to correct the defect of another; and we are not to think it extraordinary, when stars are so near together, and their motion through the field of view (in consequence of the high magnifying power we are obliged to use) so quick, that we should now and then even fall short of that general accuracy which may be had by a careful use of the micrometer.

I shall now enter into an examination of the cause of the change in the angle of position of the small star near Castor.

A revolving star, it is evident, would explain in a most satisfactory manner, a continual change in the angle of position, without an alteration of the distance. But this, being a circumstance of which we have no precedent, ought not to be admitted without the fullest evidence. It will therefore be right to examine, whether the related phenomena cannot be satisfactorily explained by the proper motions of the stars, or of the sun.

Single Motions.

(a) The three bodies we have to consider, are O, α , and x ; and, supposing them to be placed as they were observed to be in the year 1779; the angle $x \propto P$, in Fig. 1, will be $32^\circ 47'$ north preceding. We are at liberty to let the angle $P \propto O$ be what will best answer the purpose. Then, in order to examine the various hypotheses that may be formed, according to the arrangement of the principles we have given, we shall begin with No. 1; and, as this admits that all phenomena may be resolved by a proper motion of x , let us suppose this star to be placed any where far beyond α , but so as to have been seen, in the year 1779, where the angle of position, $32^\circ 47'$ north preceding

ceding, and the observed distance, near 2 diameters of the large star, required it. With a proper velocity, let it be in motion towards the place where it may now be seen at the same distance from Castor, but under an angle of position only $10^{\circ} 53'$ north preceding. It may then be admitted, that a small decrease of the distance which would happen at the time when the angle of position was $21^{\circ} 50'$, could not have been perceived; so that the gradual change in the observed angle of position, as well as the equality of the distance of the two stars, will be sufficiently accounted for. But the admission of this hypothesis requires, that α Geminorum and the solar system should be at rest; and, by the observations of astronomers, which I shall soon have occasion to mention, neither of these conditions can be conceded.

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(b) If, according to No. 2, we admit the motion of α , we shall certainly be more consistent with the observations which astronomers have made on the proper motion of this star*; and, as a motion of the solar system, which I shall have occasion to mention hereafter, has not been rigidly proved, it may, for the sake of argument, be set aside; nor has a proper motion of the star α been any where ascertained. The retrograde annual proper motion of Castor, in right ascension, according to Dr. Maskelyne, is $0''.105$. This, in about $23\frac{1}{2}$ years, during which time I have taken notice of the angle of position and distance of the small star, will amount to a change of nearly $2''.47$. Then, if we enter the short Table I have given in No. 2, with the motion αP , we find, that in the first quadrant, where the small star is placed, the distance between the two stars will be diminished, and the angle of position increased. But since it appears, by my observations, that the distance of the stars is not less now than it was in 1780; and that, instead of an increase in the angle of position, it has actually undergone a diminution of nearly 22 degrees; it follows, that the motion of α Geminorum in right ascension, will not explain the observed alterations in the situation of this double star. If,

* See Tobiaë Mayeri *Opera inedita. De motu fixarum proprio*, page 80. Also Dr. Maskelyne's first Volume of Observations. Explanation and Use of the Tables, p. iv. Or Mr. Wollaston's Astronomical Catalogue, end of the Preface. Likewise *Connoissance des Temps pour l'Année VI.* p. 203. *Sur le Mouvement particulier propre à différentes Etoiles*; par Mons. De la Lande.

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according to Mr. De la Lande's account*, we should also consider the annual proper motion of α in declination, which is given $0''.12$ towards the north, we shall find, by entering our Table with the motion αN , amounting to $2''82$, that the distance of the two stars will be still more diminished; but that, on the contrary, the angle of position will be much lessened; and, by combining the two motions together, the apparent disks of the two stars should now be a little more than one-tenth of a second from each other, and the angle of position 35 degrees south preceding. But, since neither of these effects have taken place, the hypothesis cannot be admitted.

(c) That the sun has a proper motion in space, I have shown with a very high degree of evidence, in a paper which was read at the Royal Society about twenty years ago†. The same opinion was before, but only from theoretical principles, hinted at by Mr. De la Lande, and also by the late Dr. Wilson, of Glasgow‡; and has, since the publication of my paper, been taken up by several astronomers§, who agree that such motion exists. In consequence of this, let us now, according to No. 3, assign to the sun a motion in space, of a certain velocity and direction. Admitting therefore α and x to be at rest, let the angle $P\alpha O$ be 90 degrees; then, by the 1st case of No. 3, we find that none of the observed changes of the angles of position will admit of an explanation. There is moreover an evident concession of the point in question, in the very supposition of the above angle of 90 degrees; for, if x be at the same distance as α from the sun, and no more than $5''$ from that star, its real distance, compared to that of the sun from the star, will be known; and, since that must be less than the 40 thousandth part of our distance from Castor, these two stars must necessarily be within the reach of each others attraction, and form a binary system.

(d) Let us now take the advantage held out by the 2d case of No. 3, which allows us to place x far behind α ; in which

* See page 211 of the treatise before referred to.

† *Philos. Trans.* vol. lxxiii. p. 247.

‡ See my note in *Phil. Trans.* vol. lxxiii. p. 283.

§ See *Astronomisches Jahrbuch für das Jahr 1786; Seite 259. Über die Fortrückung unseres Sonnen-Systems, von Herrn Professor Pirvoß. Und für das Jahr 1805; Seite 113,*

situation, the angle PaO will be more than 90 degrees. The star x being less than α , renders this hypothesis the more plausible. Now, as a motion of Castor, be it real or apparent, has actually been ascertained, we cannot set it aside; the real motion of O , therefore, in order to account for the apparent one of α , must be of equal velocity, and in a contrary direction: that is, when decomposed, $0''.105$ towards f , and $0''.12$, towards s . The effect of the sun's moving from O towards f , according to the 1st Table in No. 3, is, that the distance between the two stars will be diminished, and the angle of position increased. But these are both contrary to the observations I have given. The motion of O in declination towards α , according to the same Table, will still diminish the distance of the two stars, but will also diminish the angle of position. Then, since a motion in right ascension increases the angle, while that in declination diminishes it, the small star may be placed at such a distance that the difference in the parallax, arising from the solar motion, shall bring the angle of position, in $23\frac{1}{2}$ years, from $32^\circ 47'$ to $10^\circ 53'$; which will explain the observed change of that angle. The distance of the star x , for this purpose, must be above $2\frac{1}{2}$ times as much as that of α from us. But, after having in this manner accounted for the alteration of the angle of position, we are, in the next place, to examine the effect which such a difference of parallax must produce in the apparent distance of the two stars from each other. By a graphical method, which is quite sufficient for our purpose, it appears, that the union of the two motions in right ascension and declination, must have brought the two stars so near, as to be only about half a diameter of the large star from each other; or, to express the same in measures, the centers of the stars must now be $1''.8$ nearer than they were $23\frac{1}{2}$ years ago. But this my observations cannot allow; for we have already shown, that any change of more than 3 or 4-tenths of a second must have been perceived.

If, on the other hand, we place the star x at such a distance that the solar parallax may only bring it about 4-tenths of a second nearer to α , which is a quantity we may suppose to have escaped our notice in estimating the apparent distance of the two stars, then will the angle of position be above 20 degrees too large. This shows, that no distance, beyond Castor, at which we can place the star, will explain the given observations.

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(c) The last remaining trial we have to examine, is to suppose x to be nearer than α ; the angle $P\alpha O$, will then be less than 90 degrees; and the effect of a motion of O towards f , by the 2d Table in No. 3, will be an increase of the distance of the two stars, and a diminution of their angle of position. But the motion $O\alpha$, which is also to be considered, will add to the increase of the distance and counteract the diminution of the angle. It is therefore to be examined, whether such an increase of distance as we can allow to have escaped observation, will explain the change which we know to have happened in the angle, during the last $23\frac{1}{2}$ year. By the same method of compounding the two motions as before, it immediately appears, that we cannot place the small star more than about 1-tenth of the distance $O\alpha$ on the side of Castor, without occasioning such an increase of the apparent distance of the two stars as cannot possibly be admitted; and that, even then, the angle of position, instead of being less, will be a few degrees larger, at the end of $23\frac{1}{2}$ years, than it was at the beginning. This hypothesis, therefore, like all the foregoing ones, must also be given up, as inconsistent with my observations.

It is moreover evident, that the observations of astronomers on the proper motion of the stars in general, will not permit us to assume the solar motion at pleasure, merely for the sake of accounting for the changes which have happened in the appearances of a double star. The proper motion of Castor, therefore, cannot be intirely ascribed to a contrary motion of the sun. For we can assign no reason why the proper motion of this star alone, in preference, for instance, to that of Arcturus, of Sirius, and of many others, should be supposed to arise from a motion of the solar system. Now, if they are all equally intitled to partake of this motion, we can only admit it in such a direction, and of such a velocity, as will satisfy mean direction and velocity of the general proper motions of the stars; and place all deviations to the account of a real proper motion in each star separately.

Double Motion.

(f) In order to explain the phenomena of our double star, according to No. 4, by the motion of two bodies, for instance α and x , it will be required that they both should move in given
4 directions;

directions; that the velocities of their motions should be in a given ratio to each other; and that this ratio should be compounded with the ratio of their distances from O; a supposition which must certainly be highly improbable. To shew this with sufficient evidence, let us admit that, according to the best authorities, the annual proper motion of Castor is $0''.105$ in right ascension, and $0''.12$ in declination towards the north. Then, as the small star, without changing its distance, has moved through an angle of $21^\circ 54'$, the only difference in the two motions of these stars, will be expressed by the extent of the chord of that angle. To produce the required effect, it is therefore necessary that the motion of α , which is given, should regulate that of the small star, whose relative place at the end of $23\frac{1}{2}$ years is also given. Then, as α moves in angle of $53^\circ 31'$ north preceding, and with a velocity which, being expressed by the space it would describe in $23\frac{1}{2}$ years, will be $3''.51$, it is required that x shall move in an angle of $29^\circ 25'$, likewise north preceding, and with a velocity of $3''.02$. The ratio of the velocities, therefore, and the directions of the motions, are equally given. But this will not be sufficient for the purpose: their distance from O must also be taken into consideration. It has been shown, that the two stars cannot be at an equal distance from us, without an evident connection; it will therefore be necessary for those who will not allow this connection, to place one of them nearer to us than the other. But, as the motions which have been assumed, when seen from different distances, will subtend lines whose apparent magnitudes will be in the inverse ratio of the assumed distances, it is evident that this ratio, if the motions are given, must also be a given one; or that, if the distances be assumed, the ratio of the motions must be compounded with the ratio of the distances. How then can it be expected that such precise conditions should be made good, by a concurrence of circumstances owing to mere chance? Indeed, if we were inclined to pass by the difficulties we have considered, there is still a point left which cannot be set aside. The motion of the solar system, although its precise direction and velocity may still be unknown, can hardly admit of a doubt; we have therefore a third motion to add to the former two, which consequently will bring the case under the statement contained in our 7th number, and will be considered hereafter.

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(g) If we should intend to change our ground, and place the two motions in O and x , it will then be conceded, that the motion of α is only an apparent one, which owes its existence to the real motion of the sun. By this, the effect of the solar parallax on any star at the same distance will be given; and it cannot be difficult to assume a motion in x , which shall, with the effect of this given parallax, produce the apparent motion, in the direction of a chord from the first to the last angle of position pointed out by my observations; taking care, however, not to place the stars α and x at the same distance from us; using the inverse ratio of the solar parallax as a multiple in the assigned motion. For instance, let the sun have a motion of the velocity expressed as before by $3''.51$, and in a direction which makes an angle of $53^\circ 13'$ south following with the parallel of α Geminorum; and let the small star x have a real motion in an angle of $18^\circ 40'$ south preceding from the parallel of its situation, and with a real velocity which, were it at the distance of α , would carry it through $8''.89$. Then, if the distance of the small star be to that of the large one as 3 to 2, the effect of the solar parallax upon it will be $\frac{2}{3}$ of its effect upon α ; that is, while α , which is at rest, appears to move over a space of $3''.51$, in an angle of $53^\circ 31'$ north preceding, the parallactic change of place in x will be $2''.34$ in the same direction. This, though only an apparent motion, will be compounded with the real motion we have assigned to it, but which, at the distance of α , will only appear as $1''.26$; and the joint effect of both will bring the star from the place in which it was seen $23\frac{1}{2}$ years ago, to that where now we find it situated. α , in the same time, will appear to have had an annual proper motion of — $0''.105$ in right ascension, and $0''.12$ in declination towards the north; and thus all phenomena will be explained.

From this statement, we may draw a consequence of considerable importance. If we succeed, in this manner, in accounting for the changes observed in the relative situation of the two stars of a double star, we shall fail in proving them to form a binary system; but, in lieu of it, we shall gain two other points, of equal value to astronomers. For, as α Geminorum, according to the foregoing hypothesis, is a star that has no real motion, its apparent motion will give us the velocity and direction of the motion of the solar system; and, this
being

being obtained, we shall also have the relative parallax of every star, not having a proper motion, which is affected by the solar motion. Astronomical observations on the proper motion of many different stars, however, will not allow us to account for the motion of α Geminorum in the manner which the foregoing instance requires; the hypothesis, therefore, of its being at rest, must be rejected.

(h) If we place our two motions in O and α , we shall be led to the same conclusion as in the last hypothesis. The known proper motion of α , and the situations of the small star in 1779 and 1803, given by my observations, will ascertain the apparent motion of x , now supposed to be at rest. Then, since the change in the place of x must be intirely owing to the effect of parallax, it will consequently give us, in the same manner as before, the quantity and direction of the motion of the solar system, and the relative distances of all such stars as are affected by it. But, here again, the solar motion required for the purpose is such as cannot be admitted; and the hypothesis is not maintainable.

Motion of the three Bodies.

(i) There is now but one case more to consider, which is, according to No. 5, to assign real motions to all our three bodies; and this may be done as follows. Suppose the sun to move towards λ Herculis, with the annual velocity 1.

Let the apparent motion of α Geminorum be as it is stated in the astronomical tables before mentioned; but suppose it to arise from a composition of its real motion with the effect of the systematical parallax, as we may call that apparent change of place of stars which is owing to the motion of the solar system. Let the real motion of x , aided by the effect of the same parallax, be the cause of the changes in the angle of position which my observations have given. We may admit the largest of the two stars of our double star to be of the second magnitude; and, as we are not to place x too near α , we may suppose its distance from O to be to that of α from the same as 3 to 2. In this case, O will move from the parallel of α , in an angle of $60^{\circ} 37'$ north following, with an apparent annual velocity of .4536. The motion of α in right ascension, may be intirely ascribed to solar parallax; but its change of declination, cannot be accounted for in the same manner. Let us therefore

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admit that the solar velocity, in the direction we have calculated, will produce an apparent retrograde motion in α , which, in $23\frac{1}{2}$ years will amount to $2''.085$ in right ascension. But the same parallax will also occasion a change in declination, towards the south preceding, of $3''.701$; and, as this will not agree with the observed motion of α , we must account for it by a proper motion of this star directly towards the north. The real annual velocity required for this purpose, must be 1.3925 .

The apparent motion of x , by parallax, at the distance we have placed this star, will be $2''.832$ towards the south preceding; and, by assigning to it an annual proper motion of the velocity 1.3354 , in the direction of $73^\circ 10'$ north preceding its own parallel, the effect of the solar parallax and this proper motion together, will have caused the small star, in appearance, to revolve round α , so as to have produced all the changes in the angle of position which my observations have given; and, at the same time, α will have been seen to move from its former place, at the annual rate of $0''.105$ in right ascension, and $0''.12$ in declination towards the north.

In this manner, we may certainly account for the phenomena of the changes which have taken place with the two stars of α Geminorum. But the complicated requisites of the motions which have been exposed to our view, must surely compel every one who considers them to acknowledge, that such a combination of circumstances involves the highest degree of improbability in the accomplishment of its conditions. On the other hand, when a most simple and satisfactory explanation of the same phenomena may be had by the effects of mutual attraction, which will support the moving bodies in a permanent system of revolution round a common centre of gravity, while at the same time they follow the direction of a proper motion which this centre may have in space, it will hardly be possible to entertain a doubt to which hypothesis we ought to give the preference.

As I have now allowed, and even shown, the possibility that the phenomena of the double star Castor may be explained by proper motions, it will appear that, notwithstanding my foregoing arguments in favour of binary systems, it was necessary, on a former occasion, to express myself in a conditional manner,*

ner,* when, after having announced the contents of this Paper, I added, "*should these observations be found sufficiently conclusive*;" for, if there should be astronomers who would rather explain the phenomena of a small star appearing to revolve round Castor by the hypothesis we have last examined, they may certainly claim the right of assenting to what appears to them most probable.

(*To be continued.*)

XII.

On the Nature of the Varieties of Engrafted Fruit-Trees, with a Plan for increasing the Number of New valuable Fruits. By T. S. DYOT BUCKNALL, Esq. M. P.†.

TO MR. CHARLES TAYLOR.

SIR,

SOME friends have requested that I would introduce another Introduction,
Paper on the Nature of the valuable Varieties of engrafted Fruits, as they are of opinion that the Essay in the 17th Volume of the Transactions of the Society is not sufficiently extended for a subject so important to the Fruit-growers, and those interested in the productions of Fruits. As a proof of my willingness to make the Orchardist as perfect as I can, I beg you to present my compliments to the Society, with the following elucidations.

This is a subject in rural oeconomy which ought to be much Importance,
better understood than it is, in order to enable the planters to judge of the sorts proper to be planted, either as an article of pleasure, profit, or recreation; as much of the credit of the plantation must arise from judiciously choosing Trees of the best, new, or middled-aged sorts, and not of the old worn-out varieties, which latter cannot, in the planting of Orchards in common situations, ever form *valuable Trees*, and must end in the disappointment of the planter.

Engrafted Fruits, I have before said, and I now repeat, are Grafted fruits are not permanent.
not permanent. Every one of the least reflection must see that

* See Phil. Trans. for 1802, page 486.

† Society of Arts, xx. 144.

there is an essential difference between the power and energy of a seedling plant, and the tree which is to be raised from cuttings or elongations. The seedling is endued with the energies of nature, while the graft, or scion, is nothing more than a regular elongation, carried perhaps through the several repetitions of the same variety; whereas the seed, from having been placed in the earth, germinates and becomes a new plant, wherever nature permits like to produce like in vegetation; as in the oak, beech, and other mast-bearing trees. These latter trees, from each passing through the state of seedlings, are perfectly continued, and endued with the functions of forming perfect seeds for raising other plants by evolution, to the continuance of the like species.

The varieties continued by grafting last only for a time.

This is not the case with engrafted fruits. They are doomed by nature to continue for a time, and then gradually decline, till at last the variety is totally lost, and soon forgotten, unless recorded by tradition, or in old publications.

Reason, with which Providence has most bountifully blessed some of our species, has enabled us, when we find a superior variety, to engraft it on a wilding stock, or to raise plants from layers and cuttings, or even to raise up the roots, and thus to multiply our sources of comfort and pleasure. This, however, does not imply that the multiplication of the same variety, for it is no more, should last for ever, unless the species will naturally arise from seed.

Process of raising fruits, and increasing them by grafting.

Nature, in her teaching, speaks in very intelligible language, which language is conveyed by experience and observation. Thus we see that among promiscuous seeds of fruits of the same sort, one or more may arise, whose fruits shall be found to possess a value far superior to the rest in many distinguishable properties. From experience, also, we have obtained the power, by engrafting, of increasing the number of this newly-acquired tree, can change its country, give it to a friend, send it beyond the seas, or fill a kingdom with that fruit, if the natives are disposed so to do. Thus we seem to have a kind of creative power in our own hands.

From the attention lately paid to the culture of engrafted Fruits, I hope we are now enabled to continue a supposed happily acquired tree, when we can find it, for a much longer duration than if such variety had been left in the state of unassisted nature; perhaps I may say for a duration as long again,
or

or something more. After these sanguine expectations, I may reasonably be asked, to what does all this amount? for here there is no direct permanency—and why? The *why* is very obvious—because the kernels within the fruit, which are the seed of the plants for forming the next generation of trees, will not produce their like. I allow they may do so accidentally; but nothing more can be depended on.

Process of raising
fruits and in-
creasing them by
grafting.

For example, suppose we take ten kernels or pips of any apple raised on an engrafted stock: sow them, and they will produce ten different varieties, no two of which will be alike; nor will either of them closely resemble the fruit from whence the seeds were collected. The leaves also of those trees raised from the same primogenious or parent stock, will not *actually* be a copy of the leaves of any one of the varieties or family, to which each is connected by a vegetable consanguinity. I intentionally used the word *actually*, because a resemblance may be found, though not much of that is to be expected.

I beg that what has been last mentioned may not be taken as a discouragement to attempts for raising new varieties. I was obliged to speak very strongly, in order to place the culture upon its true foundation. I think it need not be observed, that there is no acquiring a new variety, but through the means of a seedling plant; and therefore whoever wishes to succeed must attempt it that way, or wait till others in their plantations may more fortunately produce it.

In choosing the seeds, that apple is most likely to produce the clearest and finest plants, whose kernels are firm, large, and well ripened. The size of the fruit is not to be regarded; for large apples do not always ripen their fruit well, or rather for cider the small fruits are generally preferred for making the strongest, highest-flavoured liquor. And from what I have been able to collect in the cider-countries, it is there the opinion, that an apple something above the improved crab promises the best success. This advantage also attends the practice: if there are no valuable apples raised from that attempt, these wildings will make excellent stocks to engraft upon.

Gentlemen who actually employ themselves in attempting to acquire new varieties, should remember that they ought to select all the sets, from the bed of apple-quick, whose appearance is in the least degree promising, and plant them together, at such a distance as to allow each to produce its fruit, which will

will happen in about twelve, fifteen, or eighteen years. My friend Mr. Knight, who undoubtedly is the first in actual exertions for procuring these happily acquired new varieties, has had two plants bear fruit at six years old, and one at five. The cider-countries have offered several premiums for procuring new varieties, and some with good effect. Premiums have been given both to Mr. Knight and Mr. Alban.

When the new variety is to be raised from a valuable *admired* apple, I should recommend the placing these seeds in a garden-pot, filled with mould from an old melon-bed; carrying the pot into a retired situation near the water, and giving attention to run the plants to as large a size as is convenient within eighteen months. With this view, the pot should be placed in the green-house the first winter; and when the plants are afterwards to be set out in the spots, they should not be placed under the drip of trees, or much exposed to the winds.

Two instances have been mentioned, the improved crab, and most admired apple; but prudence says, try all sorts, and something probably will arise; and the process is attended with little trouble or expence to a person who constantly resides in the country: yet, after all this scientific care, the apple may want flavour, and be in other respects nothing better than a common wilding.

It is an undoubted fact, and worthy of observation, that all the different trees of the same variety have a wonderful tendency to similarity of appearance among themselves; and that the parent stock, and all engrafted from it, have a far greater resemblance to each other, than can be found in any part of the animal creation; and this habit does not vary to any extent of age.

As an encouragement in attempting to increase the number of new valuable fruits, we can prove that the golden pippin is native English. The red-streak, a seedling of Herefordshire, if not raised, yet was first brought into notice by Lord Scudamore, and was for a long time called Scudamore's Crab. The Stire Apple was accidentally raised in the Forest of Dean, in Gloucestershire, and took the name of *Forest Stire*. The cider made from this apple was the strongest the country ever produced, according to any living record. The Haglo-crab, the best cider fruit now remaining, was discovered

Facts respecting
engrafted fruits,
their degree of
permanence, &c.

in the parish of Ecloe, on the banks of the Severn; and, about sixty or seventy years ago, many scions were taken from this tree by Mr. Bellamy, and engrafted on seedling stocks about Rofs. These are now grown old; and, to ascertain the age of the variety, I went with Charles Edwin, Esq. to Ecloes, in hopes of seeing the primogenious of this family. The proprietor of the estate acquainted Mr. Edwin that it had ceased to bear years ago, and was cut down. Those at Rofs are but poor bearers now, and I should suppose the variety must be 140 years old, though Marshall, who wrote in the year 1786, mentions these trees were prolific, and he supposes the sort to be about eighty years old; but, from present experience, it must be much more. The Tinton Squash-pear is of Gloucestershire; the Barland and Old-field were near Ledbury, Herefordshire. The two last pears clearly bear the names of the two fields where they were raised. The Barland fell about six years ago, visibly from weight and longevity, which was supposed to have been about 200 years. There had been many other names of estimation handed down to us, though the realities are now totally worn out, and have ceased to exist. Can any better proof be desired, that engrafted fruits are not permanent, then the regret we feel for the loss of these old valuable fruits.

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To make my paper as short as convenient, I have dwelt only on the apple and pear; yet all the engrafted fruits are under the same predicament of the seed not producing its like, and the offspring in time falling into a nothingness of growth and bearing, though that space of time must certainly depend on the natural longevity and hardness of the sort, soil, position, care, &c. All these are more fully expressed in the papers published in the different volumes of the Transactions of this Society, and the two volumes of the Orchardist, wherein the whole system is extended, to form a rational culture for the management of Standard Fruits.

It should be remembered, that as I am now alluding to the state of actual permanency, fifty years are to be accounted as nothing; and as often as we come to that point, we are compelled to resort to our first assertion, "That engrafted fruits are not permanent, they being continued from elongations, and not raised as a repetition of seeds." This is the only rational

Facts respecting rational way as yet introduced of accounting for the loss of engrafted fruits, the valuable old varieties of fruits. Should a better system be their degree of permanence, &c. introduced, I shall readily adopt it; but this sufficiently answers the purposes of the planter.

Some years ago, from due investigation and thorough conviction, I propagated this principle; and it was published in the 17th volume of the Society's Transactions, in the following words: "All the grafts taken from this first tree, or parent stock, or any of the descendants, will for some generations thrive; but when this first stock shall, by mere dint of old-age, fall into actual decay, a nihility of vegetation—the descendants, however young, or in whatever situation they may be, will gradually decline; and, from that time, it would be imprudent, in point of profit, to attempt propagating that variety from any of them. This is the dogma which must be received. I do not expect a direct assent, neither do I wish it, for it should be taken with much reserve; but it is undoubtedly true." These considerations should stimulate us in searching after new varieties, equal, or perhaps superior to those of which we regret the loss.

Observe that, from the time the kernel germinates for apple-quick, should the plant be disposed to form a valuable variety, there will appear a regular progressive change, or improvement, in the organization of the leaves, until that variety has stood, and grown sufficient to blossom and come into full bearing; that is, from the state of infancy to maturity; and it is this and other circumstances, by which the inquisitive eye is enabled to form the selection among those appearing likely to become valuable fruits. But from that time the new variety, or selected plant, compared with all the engraftments which may be taken from it, or any of them, these shall shew a most undeviating sameness among themselves.

It is readily allowed, that the different varieties of fruits are easily distinguished from each other by many particulars, not only respecting their general fertility, and the form, size, shape, and flavour of the fruit, but also the manner of the growth of the tree, the thickness and proportion of the twigs, their shooting from their parent stem, the form, colour, and consistence of the leaf, and many other circumstances, by which the variety can be identified; and were it possible to
engraft

engraft each variety upon the same stock, they would still retain their discriminating qualities, with the most undeviating certainty.

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their degree of
permanence, &c.

The proper conclusion to be drawn from the statement in the last paragraph, is this—that were any one to put the thought in practice on a full-grown hardy or crab stock, it would produce an excellent proof that engrafted fruits are not permanent. For if twenty different varieties were placed together, so that each might receive its nurture from the same stem, they would gradually die off in actual succession, according to the age or state of health of the respective variety, at the time the scions were placed in the stock; and a discriminating eye, used to this business, would nearly be able to foretell the order in which each scion would actually decline. Should it also happen that two or three suckers from the wilding stock had been permitted to grow among the *twenty grafts*, such suckers or wilding shoots will continue, and make a tree after all the rest are gone. A further consequence would result from the experiment: among such a number of varieties, each of the free growers would starve the delicate, and drive them out of existence only so much the sooner. It must be observed, that this supposed stem is the foster-parent to the twenty scions, and real parent to the suckers; and those the least conversant with engrafted fruits know the advantage acquired from this circumstance. And here it is worth while remarking, that a Gascoyne, or wild cherry, will grow to twice the size that ever an engrafted cherry did.

By an experiment we have had in hand for five years, it will appear that the roots and stem of a large tree, after the first set of scions are exhausted or worn out, may carry another set for many years; and we suspect a third set, provided the engrafting is properly done, and the engrafter chooses a new variety. Now the Ripston pippin, of Yorkshire, is the favourite, as being a free grower and good bearer, with fine fruit. This however may be certainly depended on, that when a new apple is raised from seed, if a scion were placed in a retired situation, and constantly cut down, as a stool in a copse-wood, and the apple never suffered to fulfil the intentions of nature in bearing fruit, the practitioners of the following ages may secure scions from that stool, to continue the variety much longer. Hence, though I have written as much as is in

my

Facts respecting my power against permanency, yet I have taken some pains engrafted fruits, to assure the planters, that forecast, selection, pruning, cleaning, their degree of permanency, &c. lines, and care, will make the orchards turn to more profit for the rising generations, than what they have done for the last hundred years.

To place the nature of varieties in its true light, for the information of the public, I must maintain, that the different varieties of the apple will, after a certain time, decline, and actually die away, and each variety, or all of the same stem or family, will lose their existence in vegetation; and yet it is a known fact, and mentioned in the 17th volume of the Transactions, that after the debility of age has actually taken possession of any variety, it will yet thrive by being placed against a southern wall, and treated as wall-fruit. Who, however, can afford to raise cider at that expense, except as matter of curiosity, to prove, that when the vital principle in vegetation is nearly exhausted, a superior care and warmth will still keep the variety in existence some time longer?

It should be understood, that the external air of Britain is rather too cold for the delicate fruits, which is the reason why, in the Orchardist, I lay such a stress on procuring warmth for the trees, by *draining, shelter, and manure*. It would be now lost time to attempt to recover the old varieties as an article of profit.

If I have not expressed myself, in this Essay on the Nature of Varieties, with so much clearness and conviction as might have been expected, it should be considered that it is an abstruse subject, very little understood, and requiring at first some degree of *faith, observation, and perseverance*. The prejudices of mankind revolt against it. They are not disposed to allow the distinction of nature; and they imagine, that in the act of engrafting or multiplying they give new life, whereas it is only continuing the existence of the same tree, stick, or bud. Observe what I said before:—the seed of the apple, when placed in the earth, germinates, and unfolds itself into a new plant, which successively passes through the stages of infancy, maturity, and decay, like its predecessors. I might say, all created nature is similar in this respect; though, from the circumstance that varieties are much longer-lived than man, the plants have appeared to be possessed of eternal

eternal powers of duration: nothing sublunary, however, which possesses either animal or vegetable life, is exempt from age and death.

Facts respecting
engrafted fruits,
their degree of
permanence,
&c.

Within the last twenty years I have travelled many hundred miles, and conversed with the most intelligent men in each country; and I now want to convince mankind, for no other reason than because it is their interest so to believe, that there is in creation an order of beings (engrafted fruits) so formed, that we have the power of multiplying a single variety, to whatever number of trees we please;—that the first set arises from a small seed;—that the next and descendent sets are propagated by engraftings, or from cuttings, layers, &c.;—and that although these trees may amount to millions, yet, on the death of the primogenious or parent stock, merely from old-age, or nihility of growth, each individual shall decline, in whatever country they may be, or however endued with youth and health. I say they shall gradually begin to decline; and in the course of time, or of centuries, to those who would prefer that expression, the *whole variety* will scarcely have a single tree remaining to show what the fruit was. Let those who are not disposed to assent to this statement, ask themselves what is become of the old lost varieties? did they die, or did wicked men maliciously cut them up?

I, who am firmly convinced of the truth of what I have advanced on this subject, have no doubt but that the same would happen by engrafting on the Oak or Beech, if the mast raised from the engrafted tree did not produce the like; for there the question turns.

Is it not known, that the woodman, in setting out his sapling oaks, always selects new seedling plants, and never continues one upon an old stool; and that if he should so blunder, that tree, from the stool, will neither have the freedom of growth, nor the size or firmness of timber, equal to a new-raised plant.

I wish I could persuade my friends, that, with the same attention with which the woodman acts, the planter is to raise his orchard from the young fruits which thrive in the neighbourhood, or are in health and full bearing in the country whence they are to be brought.

The fruit-grower should look to selection, cleanliness, and care. To me it is a circumstance perfectly indifferent, whether

Facts respecting their degree of permanence, &c. ther he is to use Mr. Forfyth's composition, Mr. Bulingham's engrafted fruits, boiled linseed oil, or my medication. I only maintain that the wounded parts of trees want something to destroy the insects and vermin, and heal the wood, from which the trees are kept in health.

Let those who are blessed with fruit-plantations attend to their preservation, and not leave them to the state of unassisted nature.

I am, Sir,

Your most obedient Servant,

THO. SKIP DYOT BUCKNALL.

Hampton-Court.

12th Oct. 1801.

XIII.

Explanation of the Subjects concerning which Questions were proposed at Page 71 of the present Volume; namely, 1. The Place of the Erect Image behind a Concave Speculum; 2. Of the Image formed by a Concavo Convex Mirror, which is not Left-handed, and has the Property of revolving along with the Mirror; and 3. The Figure of the Sky. By W. N.

Answers to certain questions.

AS the questions of my correspondent R. B. have not been treated by any other of my friends, I shall here discuss them according to my promise.

Erect image in a concave mirror.

1. The question respecting the erect image in the concave mirror relates to a phenomenon which formed the subject of discussion in print very early in the last century, though I cannot at this moment recollect the authors who have related the facts and arguments. R. B. is perfectly correct as to the various divergence of the rays composing the pencils which issue from radiant points at different distances from the eye, but it does not appear that our notions of distance are greatly re-

Our judgment of distance is not founded on the nature of the pencils of light;

gulated by that circumstance. Our judgment of the distances of near objects is partly founded on the adjustment required to be made in the eye itself to produce distinct vision by the rays of the several pencils, as R. B. remarks, but much more on a circumstance commonly noticed by authors, but by him overlooked, namely, the degree of convergence between the optical axes of the two eyes which is required to avoid squinting,

or

or the phenomenon of two images. In proof of this, if he will take two pens or pencils or other pointed objects, and, shutting one eye, endeavour to apply their points end-wise to each other by moving them across the line of sight, he will find how much less precisely one eye can give a judgment on distances, than both eyes. And our judgment of distant objects is established not at all upon the nature of the pencils, but on the perspective arrangement or angular magnitude of the objects together with their gradual obscuration (from the interposed mass of air) called aerial perspective. Hence we find that the several parts of a well drawn sketch of mere lines, and still more of an excellent painting, give all the notions of distance, though there cannot be any difference in the divergence of the pencils of light to correspond with that effect. The consciousness that a body is approaching or retiring arises little if at all from any change in the pencils; in near objects that notion is rather gained from the required change in the optical axes, and in these and all distant objects it is almost entirely produced by the angular magnitude becoming larger or smaller, while we contemplate the appearances.

These truths will explain the image in the concave mirror: For 1. When the face is near the mirror, the image is seen by a great convergence of the optical axes, which becomes less and less as the observer retires, at the same time that the image itself subtends a less and less angle:—It therefore, for both reasons, appears to retire. 2. At a greater distance the image seems stationary while the observer retires, because the angular enlargement of the image is nearly compensated by its diminution from increased distance; and 3. At a still greater distance the angular enlargement increases so fast that the image seems rapidly to come forward until it is lost in confusion.—If both eyes be kept open, the observer has a considerably accurate notion of the real distance of the frame, which, together with the confusion of two different images presented to the eyes, may lead him to have the sentiment of a mere angular enlargement; but if one eye be covered, the notions of retreat, stationary position, and rapid advance of the image will infallibly be adopted.

2. With regard to the concavo-convex mirror, none of the pencils of light are brought truly to a focus. If we conceive the surface of the mirror to be divided into zones by lines

Explanation of
the effects of a
concavo-convex
mirror,

drawn perpendicularly across the shortest arc of convexity that passes through the vertex; and these lines to be indefinitely near each other;—or to speak less mathematically, if the mirror were divided into small parallel stripes, drawn right across its concavity;—each of those small zones or stripes will reflect the light of any distant object to a focus before the mirror without any sensible error; whence it will diverge in a kind of flat pencil; and these pencils themselves will not be parallel to each other, but will diverge from a point behind the mirror or virtual focus; so that this last divergence of the pencils will be at right angles to the divergence of the rays that compose them. If therefore we attend to the portion of light which enters the eye from what may be taken to be a radiant point of the image, in this case, we shall perceive that they cannot be brought to a correct focus on the retina. For if the eye be adjusted to the place of convergence before the mirror, the focal spot will be elongated in the direction of the arc of convexity; and on the contrary, if the eye be adjusted to the virtual focus of the convex, or point of divergence, the rays of the pencils will not be duly collected, but will render the spot on the retina oblong in the direction of the arc of concavity. Nevertheless, if the distance of the eye be so great, as that the difference of adjustment for these two points shall not produce any sensible effect of this kind, the whole image will appear sufficiently neat and distinct, though a little deformed; that is to say, those dimensions which are governed by the concavity, will be somewhat greater than those governed by the convexity; because as we may affirm, the concave mirror is nearer the eye than the convex. From this general explanation, it will be easily understood that the image must disappear and become utterly confused by the quantity of cross aberration, when any attempt is made to examine it with a convex lens; and that it will be impossible to form an image of the sun or of any other object upon the surface of paper or other similar material. In fact the reflected light from the sun considered as a luminous point, will converge before the mirror to a line or caustic curve of which I have not yet considered the properties; but which, if the concave mirror were cylindrical, would be a right line at the distance of half the radius from the vertex.

It is not necessary after what has been said to enter into any fuller explanation of the reason why the image is inverted, (that is to say either right handed or upside-down) with regard

to

to the relative position of its parts considered across the concavity, and the contrary along the lines of convexity;—or, while these lines have their position altered by rotation of the mirror, the image itself will also appear to revolve with twice the velocity. These effects, as well as that of the extreme distortion of the image, seen when the object is within the centre of concavity, will be easily apprehended by the reader, whose notions of optics are sufficient to enable him to understand the preceding paragraph.

3. The enlarged appearance of the sun and moon at low altitudes or near the horizon, and the apparent flatness of the concavity of the sky have been explained, as my correspondent observes, by reference to the diminished light of the heavenly bodies, when it passes through a long portion of the atmosphere, and likewise to the notions of distance which are obtained by looking over a long row of terrestrial objects. That is to say the explanation is grounded on the supposition or notion of greater distance along the horizontal line deduced from consideration of the aerial and the geometrical perspective. The observations and objections of R. B. appear to me to be perfectly well founded. I think the effect is produced almost entirely by the geometrical perspective, not of the houses, trees, and other land objects, but of the clouds themselves. When the clouds are in low distinct flakes with clear openings between them, the angular magnitude both of the clouds and their intervals will be greater near the zenith, and will diminish as the zenith distance encreases, so as on some occasions to exhibit the appearance of objects running out to an immense distance in the concavity of an extremely flat dome. On the contrary, when lofty towering masses of clouds rise from the horizon to a considerable elevation facing the setting sun, or otherwise so circumstanced that the tint of the whole mass shall be very little varied, the mind, so far from adopting the notion of an extended dome in that part, shall receive the impression of an immensely elevated wall, with very little curvature as it rises. Both these conditions are not unfrequently to be seen at one and the same time, and the sky is then, according to the observation of R. B. very far from exhibiting a figure of regular dimensions. A great variety of intermediate forms and groups of the clouds often present themselves, by which the apparent figure of the sky will be made to differ from these extreme cases. When the sky is perfectly and closely clouded, we have

Why the sky appears flat, and the sun and moon large in the horizon.

have perhaps very little notion of an apparent figure unless from our former habits, or from the angular motion produced by the wind. This will be greatest near the zenith, and by shewing us that the clouds over our heads are nearer than the others, will give us a conviction of the figure of a flat arch.

Useful Notices respecting various Objects. By a Correspondent.

Cotton in Ink.

R. B.

Cotton in ink recommended.

THE ancient practice of putting cotton in ink is almost entirely given up. But when we consider that the colouring matter of ink is merely a precipitate, in the act of slow subsidence, and that the gum as well as the Gallic acid, are subject to speedy decomposition or mouldiness by exposure to the air, we shall see good reasons for resuming it. The black fecula is kept suspended by the cotton; the fluid is prevented from circulating by heat, cold or mechanical agitation, and consequently presents a very small surface to the air; and lastly the method of dipping tends to keep the pen clean and in good condition. I can take upon me to assert that ink in cotton is blacker, more durable, and much less liable to become mouldy. The latter effect may be almost entirely prevented by occasionally turning the cotton upside down, that is to say, every two or three days

Shaving with Water.

Shaving with water.

IN some of your early numbers the operation of shaving has been rather amply discussed by yourself and correspondents. I have nothing to add in the way of reasoning to what has been there brought forward; but I take this opportunity of noticing a fact. Long ago it was observed to me that the soap answers no other purpose than that of clearing the skin, in proof of which it was asserted that the face so cleaned may be shaved quite as well without the lather as with it. I made trial of this process, but did not find it succeed, because I wiped the skin dry. But I have since found that if the beard be well washed with soap, and then with clear water, the operation of shaving may be very effectually and pleasantly performed, while the water continues upon the skin. The effects are that the razor cuts more keenly and closely, and the skin seems to be much less tender. I leave your other correspondents to reason and discover the cause of the excellence of this method; in the mean time I shall continue to use it, unless a still more considerable improvement should be made.

*Digester in which the Heat,
and pressure, are rendered stationary.
By Sir A. N. Edelerantz.*

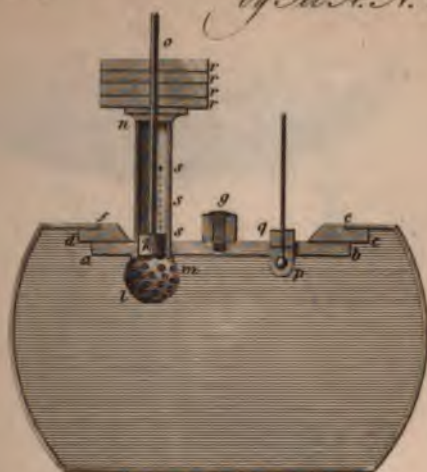


Fig. 1.

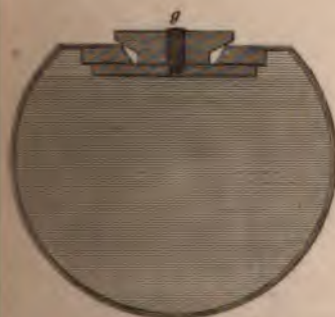


Fig. 2.



Fig. 3.

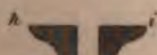


Fig. 4.

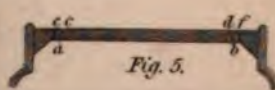


Fig. 5.



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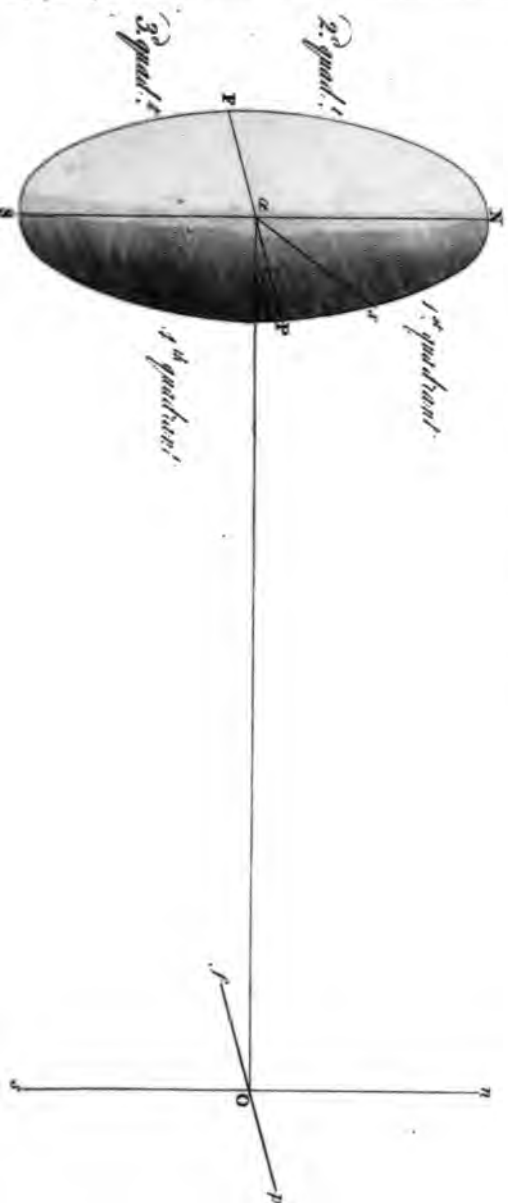
ASTOR, LENOX AND
TILDEN FOUNDATIONS

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1892

*On the Motion of the Tangent in the
 Motion of a Circle.*

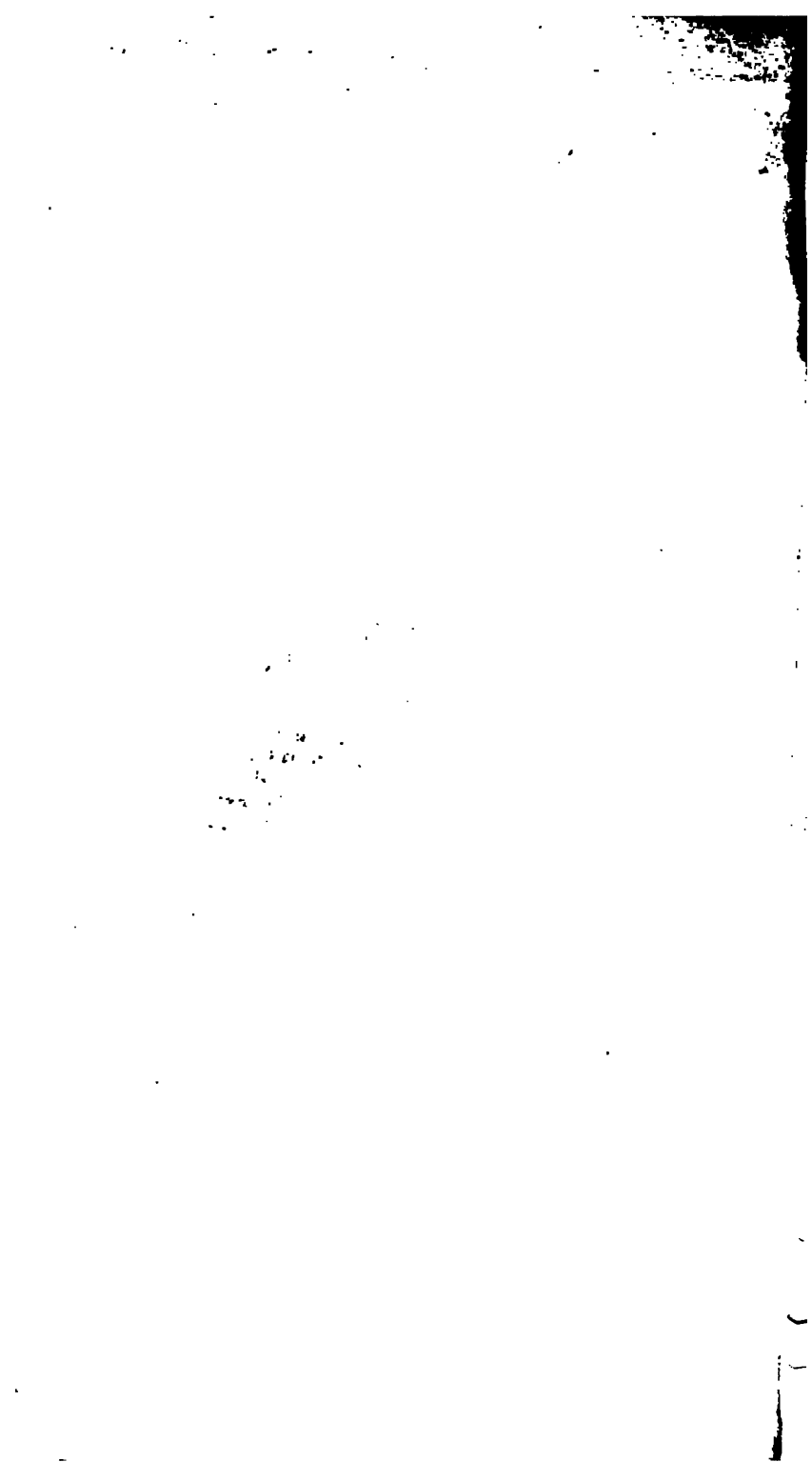




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Memorable Events of 1781.





Mercurial Air Pump
by Sir A. N. Edelcrantz.

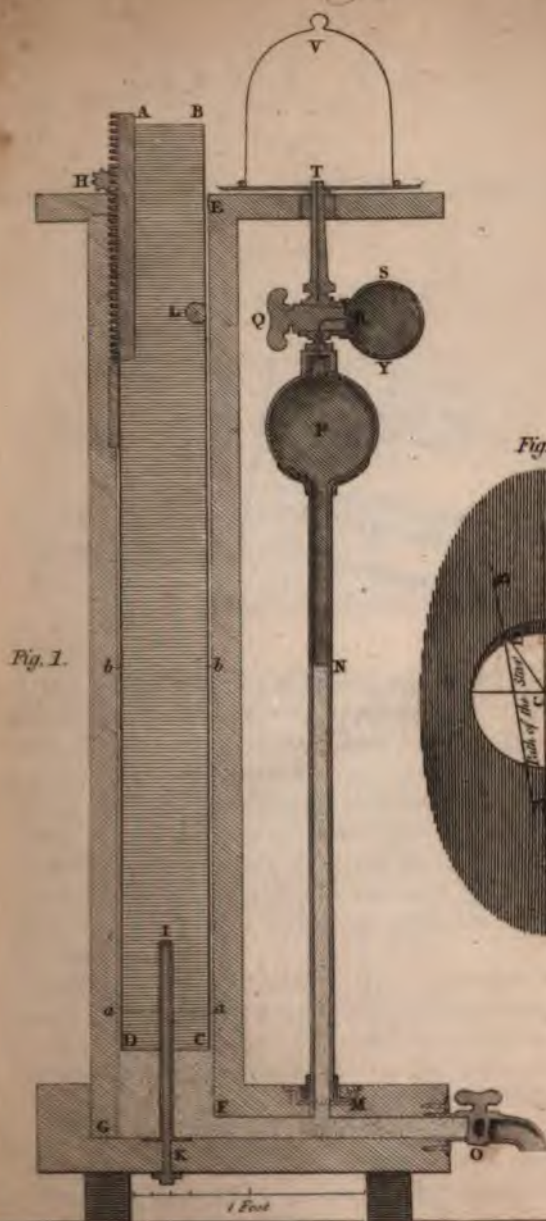


Fig. 2.



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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

A P R I L, 1804.

ARTICLE I.

*Experiment proving the Advantage of Periscopic Spectacles. Communicated in a Letter from W. H. WOLLASTON, M. D.
F. R. S.*

To Mr. NICHOLSON.

SIR,

THE opinion given by Mr. Jones, in your last Journal, respecting the improved form of spectacle-glasses, on which I had delivered my sentiments in the number preceding (p. 144,) induces me to address you once more upon that subject. Mr. Jones's opinion of the improved spectacle glasses,

It appears wholly unnecessary to follow Mr. J. through his various observations, or to remark upon the experiment by which he deceives himself; because all doubt of the advantage of the periscopic glasses may be removed by the following direct comparative trial, which any person who chooses can make without difficulty. referred to experiment.

I have before me two glasses, each of four inches "positive focus," as proposed by Mr. J. the one double-convex, which in his judgment is pronounced to be "indubitably the best and most convenient that can be devised" (p. 194); the other a concavo-convex, or meniscus, which he thinks "evidently the worst of the two for a spectacle-glass," (p. 197.) With the form of lens recommended by Mr. Jones,

When I fix the former at the distance for most distinct vision opposite to a printed octavo page, and approach my eye to the the extent of distinct vision compared with
VOL. VII.—APRIL, 1804. R glasses, that by the

periscopic glass
was as 24 to 40,

glass, I cannot without pain read quite twenty-four lines; but upon substituting the periscopic glass fixed in the same position, I can with *ease* discern every word in the page, which contains forty lines.

—or rather as
1 to 3.

The enlargement of the field of view observable in this trial is sufficient to evince the superior utility of the periscopic glass; but were there occasion to compare more nearly the circular areas that may be seen with equal distinctness, they would be found to differ by a ratio as great as that of three to one.

A corresponding
advantage in
shallower lenses.

This difference is of course more evident in glasses of so high power than in those generally used by long-sighted persons for common purposes; but it cannot be doubted, that a corresponding, though smaller, inequality subsists wherever there is the same dissimilarity of construction, even when the focal distance is longest.

Conclusion.

The advantage in question is therefore indisputably proved by direct experiment; to the *novelty* of it Mr. J. himself has unintentionally contributed very satisfactory evidence; but as to its importance, those alone, who have the misfortune to labour under any defect of vision, must ultimately decide.

I remain, Sir,

Your obliged humble servant,

March 20, 1804.

W. H. WOLLASTON.

II.

Additional Observations on some remarkable Strata of Flint in the Isle of Wight, in a Letter from Sir CHARLES ENGLEFIELD, Bart. F. R. S. to JOHN LATHAM, M. D. F. R. S. and L. S. (See Page 183 of the present Number.)*

DEAR SIR,

Observations on
remarkable
strata of flint in
the Isle of
Wight.

I FEEL much flattered at the notice taken of my Paper on the chalk pits in the Isle of Wight by the Linnean Society; and as I wish to render my account of the very curious appearances observed by me in them as perfect as I can, the following additional observations on the subject made during a second visit last year are, by your favour, submitted to the Society.

* Linnean Transactions, Vol. VI.

The pits I last year inspected are as follow, beginning from the east:

Brading pit, which is at the eastern point of the great ridge, where the valley of Brading-haven intersects it, and separates it from the Yaverland-hill, which terminates in the sea at Culver and Bembridge. A road cut into the chalk above Knighton. Ashley-down pit, about three miles east of Newport. A pit very near to and south of Carisbrook castle. The cliffs and caves of Freshwater bay, both east and west of the valley which intersects them entirely and runs from Freshwater to Yarmouth.

The Yaverland chalk is therefore the only part which I have not examined; and little doubt can be entertained of its similarity to the rest of the range, to which it evidently belongs.

In Brading pits some flints appear in detached nodules, and these are found and unbroken.

The inclined strata of flint are visible, but not to advantage, owing to the manner of working the pits. In these strata the flints are universally shattered, some into absolute powder, others into grosser powder and fragments mixed. But besides these strata, the chalk in this pit is divided by vast perpendicular fissures, as smooth as plaster walls, and in some of these fissures flint has formed, which appears broken like that in the strata.

The road above Knighton only just cuts into the chalk stratum, but all the flints visible in the banks are extremely shattered.

The pit at the west end of Ashley-down, near two large barrows, is the most extensive and satisfactory of any I have seen. The perpendicular face of the chalk, where worked, is not less than fifty or sixty feet, and its direction is at right angles to that of the strata, and parallel to their line of dip:—of course they are seen to very great advantage. The strata seems to dip northward more rapidly than in any other place where I could observe them. The angle of inclination is from 75 to 80 degrees. There are not layers of flints between every layer of the chalk. Some of the chalk is peculiarly solid, and rises in very large masses affecting a cubic form. Their solid vein is from twenty-five to thirty feet thick, and is in strata from three to four feet. In all this solid part there are very few flints.

Both above and below this harder bed (speaking of the original position of the strata) the chalk is softer, and has more

Observations on flints in it. The stratified flints in this pit are full as much remarkable shattered as any I had seen. The nodules are not at all broken. strata of flint Many of the stratified flints are much defaced in this pit by an in the Isle of admixture of pyrites, so as to be quite opaque like a coarse Wight. jasper, and these flints are much softer than the others, as is always the case in the impure flints.

In the chalk pits near Carisbrook the strata are not so visible as in the pits north of the castle (described in the first papers) but the flints are to the full as finely, though perhaps not so generally broken. In one flint I observed that though it lay in its bed undisturbed, chalk as if in a fluid state had run into one of the fissures. Every appearance of this pit indicates that the chalk, since its stratification, has received a most violent shock.

The chalk at Freshwater bay appears in high perpendicular cliffs, particularly on the western side of the bay. Both on the east and west the strata dip northward near 80 degrees, and the dip seems to run east and west very regularly. The western cliff has a very regular and perpendicular face to the eastward; and here the parallel direction of the strata, each separated by a thin line of black flint presents a most curious appearance. The flint here is often found in thin plates of considerable extent, sometimes not above an inch thick, and seems formed from each side of the space which it fills, as the exterior parts (or those nearest the chalk) are the purest and blackest, and it is gradually whiter towards the middle, where there is often a soft line of chalk included between the two plates of flint. All the stratified flints are more or less shattered, and some are reduced to very fine powder. The cave at Freshwater, which is really a beautiful as well as a very curious one, is formed by the action of the sea on these nearly vertical strata. They are of different hardness, and all intersected with fissures at right angles to the strata. When the sea acts on and wears away a soft stratum, a gallery is formed, and the upper parts of the stratum between fissure and fissure drop out, much in the same way as bricks are apt to do out of the flat arch over a window, the harder contiguous strata serve as walls to the gallery, but are by degrees perforated in different parts, and become irregular pillars, supporting the vast weight of the hills above, until the action of the sea weakens them so far that they fall, and a part of the face of the hill goes with them, so that the cave is constantly, although slowly, changing its form.

Larger masses of the harder strata, defended by their flint coating, also stand up in the bay as insulated rocks of different shapes, and much resembling the Needle Rocks, which are exactly of the materials and formed by the same process. In this part of the chalk stratum, I saw several fossils remain which I had sought in vain in the pits I had visited. One was singular, it had the appearance of part of a very large shell, regularly striated, and almost flat. I have often seen small fragments apparently of a similar shell in chalk, but never a large piece. It was so firmly fixed as not to be removed without a chisel, which I had not.

Observations on remarkable strata of flint in the Isle of Wight.

To these observations on the chalk of the island, I must add, that this whole range, although really chalk, is much harder than the chalk of the South-downs, inasmuch that the carpenters cannot use it for drawing lines, but import chalk for that purpose from Portsdown hill above Portsmouth. They also call the island chalk by the name of marle, which is, however only the British name for chalk, and appears in many compound names, such as Marl-borough on the Wiltshire chalk hills, and the very significant one of Albemarle, or white chalk.

As I have made some further observations on the southern range of hills which form the back of the island, I will trespass on your patience a little longer, particularly as they in some degree contradict, or rather correct, what I had advanced on that subject in my former letter.

When the northern front of those hills is viewed from Ashley down, the stratum of stone mentioned in my former letter as lying directly under the chalk of St. Catharine's and Dunnose hills, appears every where to maintain an horizontal position; and so in its general position, particularly in its northern front, it certainly does, but just behind the village of Ventnor, the stratum entirely disappears, as if it had been ingulphed in a great chasm, and a deep and narrow valley runs winding into the chalk hill of St. Boniface, though it does not penetrate through it, which seems the remains of the fissure into which the stone had sunk.

The appearances of the great stone stratum, from Niton eastward to Ventnor are noted as follows in the journal made on the spot.

On an attentive inspection of the strata of the under cliff, it appears that the great stratum of rugged and laminated stone, which

Observations on
remarkable
strata of flint
in the Isle of
Wight.

which first appears at the west side of St. Catharine's, and thence ranging eastward, forms the front of the cliff, overhanging the Underway dips in its southern face gently to the eastward. The cliffs at Mirables are much higher above the sea than those at St. Laurence, and from thence they decline, till at the opening of the hill above Ventnor they totally disappear. A small crag just peeps out of the eastern face of this dell, and the whole hill of St. Boniface is, as far as can be seen, composed of chalk. As however this, like all other chalk hills, is in the state of a steep slope, covered with turf, perhaps by giving into its face the stony stratum might be discovered. It is also to be observed that the chalk, which is not visible above the rock at Mirables, begins to appear soon after, and grows gradually thicker as it proceeds eastward. At St. Laurence it forms a thick cap to the rocks; and at Steephill Shute, its thickness is very much increased, and soon after nothing but chalk appears in St. Boniface's hill. In what form the rock reappears at Dunnose to the east of St. Boniface, I have had no opportunity of examining.

I should not, dear Sir, trouble you with these defective observations, but that every notice, however imperfect, may be of use when connected by future observations, and that they may serve as a stimulus to other travellers, who often go over this beautiful line of country, to turn their attention to its singular natural phenomena.

I send you two specimens of the broken flints, one from Brading, the other from near Carisbrook, but the tickets are mislaid, and I am not sure which is which,

I remain, &c.

Tilney-Street, May 26, 1801.

III.

*Account of a Machine for sweeping Chimnies, by a Blast of Air.
In a Letter from Mr. J. C. HORNBLLOWER, Engineer.*

To Mr. NICHOLSON,

DEAR SIR,

Introduction.

I SHALL be much obliged if you will allow me a page or two in your Journal for a description of a machine I have lately got up for the purpose of cleansing chimnies from soot. My

with is, if possible, to keep alive the spark that has been kindled by a truly philanthropic individual, with a view to restore a degraded class of our species to their rank in civil society. The machine, as it is constructed solely for the purpose mentioned above, is certainly novel in its intention, and without a better acquaintance with its principle and operation than the public are at present in possession of, it is not likely to gain that notice which in my judgment it is entitled to. There is a propensity in us to turn into ridicule any thing that will admit of a ridiculous construction, or afford an opportunity for the display of our wit; and such is the unfortunate circumstance of this machine, that grinning takes precedence of gravity the moment it is announced as a candidate for the gold medal at the Adelpi. However, happy for it, and thrice happy for me, there is a tribunal which can judge fairly and scientifically of its merits, for which I present it as follows.

The body of the machine A, *Plate XIII.* is made of copper, of about 3lb. to the square foot, and its contents are about three cylindrical feet. In the middle of the cover, which is foldered on, is a syringe or condenser, having its handle as appears above the cover. On one side of the cover is inserted a crooked pipe, having a valve opening inward in its inner or lower end; the stem of which comes up through the pipe, and terminates with a button C. The pipe is continued by a flexible leather one, to which is united the tin plate tube B B.

Propensity to
ridicule inven-
tions.

*Description of
the apparatus.*
A vessel, into
which air is
condensed, com-
municates with
a tube charged
with small
gravel, which
being blown up
the chimney
brings down the
soot.

This tube B B has a cross bar of tin plate fixed edge-ways in the bottom or breech of the tube which detaches from the leather pipe somewhat like a pistol barrel, leaving a piece of tube about six inches remaining to the leather pipe, into the bottom of which this cross bar is fixed, and serves to receive a charge of small gravel, having a piece of paper first laid on the cross bar; the other part of the tube is then to be replaced, and the air-vessel supposed to be full; the valve is pressed down by a little lever accompanying the machine, and its contents are discharged into the shaft of the chimney; and if there is any soot worth sweeping away, it will come down.

Observations concerning the Machine.

I do not pretend to have made any new discovery in the construction or application of the machine, for it is well known that a musket has often been applied for the same purpose, and

This operation
is similar to
firing a musket
up the chimney,
with

with as good effect as need be, barring the hazard of setting fire to the foot; and indeed, when a chimney has accidentally taken fire, it is the best and most expeditious mode of putting it out, at least of bringing the foot into a situation by which it may be extinguished.

Estimate of the velocity of the extended air, and its effect in the chimney; together with that of the gravel.

I have said that the vessel contains nearly three cylindrical feet, and we cannot crowd three atmospheres of air into it, in which case there will be 42 lbs. per inch square at round numbers, pressing against the charge in the tube, or rather against the valve, the tube being $2\frac{1}{4}$ inch diameter, which amounts to 168 lbs. for the whole area. This air will all be discharged in one second, the mean velocity of which may be fairly reckoned at 50 feet, in that time having impetus in itself sufficient to carry away any foot of consequence in a chimney of 100 feet high; but when we take into the account the charge of gravel, being alternately incident and reflected on all sides of the chimney, we need not fear to affirm that it is perfectly applicable to its intention.

Additional tubes for curved chimnies.

To render it as universal as possible, there must be another tube, to be occasionally used, when the side of the chimney near the fire place is gathered over in order to bring the throat of the flue over the fire, as at *Fig. II.* from *a* to *b*, or it should be in several pieces, to conform to the height of this gathering, and in this case the charge must be at the upper fissure at *b*, and the pipe stayed as perpendicularly as can be conveniently done; for which purpose there is a piece made to fix on the pipe, having two stems, as *Fig. III.* which if put in the bearing side of the pipe, will keep it upright and in the crater of the flue. The machine thus constructed is to be laid on a little truck, and transported from house to house, with a gallon of gravel to begin with; because until it has obtained the good opinion of the public, it would not be necessary to attempt such regulations as would afterwards be deemed requisite to facilitate the operation.

Trial at the house of the Society of Arts in the Adelphi.

This poor thing, brought forth under other disadvantages besides its singularity, was conducted to the Adelphi, to undergo an eventful trial, in competition with some others, who had no such singularity to be imputed to them; they being all descendents of the stock of one family, the remotest relation between each not being more distant than cousin-german.—

Other machines also exhibited there.

One of them however possessed great originality and ingenuity in

in the design. It consisted of a congeries of brushes, which in preparation for action were collapsed by their construction so as to admit of being thrust up the chimney with very little resistance, and when it had gained the top, on pulling a string, it expanded to the dimensions of the chimney, and brought the foot down before it; but from want of a mechanical attention to its construction, some parts being too strong, and some too slight, it broke in the first experiment.

Another (very elegant instrument) was presented to the committee, consisting of a large brush, such as I have seen in some churches to sweep down the cobwebs, with a scraper attached to it; lengthened to the height of the chimney by a series of whalebone rods, whipped together as in the construction of waggoners' whips, very accurately jointed to each other. Indeed this was essential and common to them all, except one; and that was a brush, not to be sent up the chimney, but down it, by getting on the top of the house, and putting it into the chimney, and so letting it fall to the bottom. However, it was said, they would all do very well but mine, as a machine like that was adopted to a rectilinear flue only; whereas theirs had so many joints and other properties to conform to a curvature, that they would be sure to go through a crooked flue as well as a straight one.

When the late Doctor Johnson had any thing advanced to him of the marvellous kind, his way of hesitation to admit it, was by saying "It may be very true, but it is impossible."—So I say with respect to the disposition of these rods, when set in competition with a volume of air going at the rate of fifty feet in a second from the lower part of the chimney to the top. What is there to stop it? What is there to prevent its conformity to the curvature or angles, if you will?

The manner in which these brushes act, let their construction be what it may, is by striking off the foot. What does a boy within the flue with the brush in his hand? To be sure sweeping is the most appropriate term for what he does; but then, unless, we attend to the peculiarity of the action, and consider how far it is possible to effect that sweeping by another mode of action, we shall not get any further than the boy in the flue. But is it not as easy to conceive of a quantity of air being put into action along a road or a street, so as to sweep away the dust before it? No; you will say, it blows it along; but

Brushes.

A single brush.

Notion that the author's machine is suited only to a straight flue.

The same denied.

To blow down the foot is as natural as to sweep it down.

but the term blowing associates only with the accustomed notion of the action of wind, and was the effect to be produced by the agency of steam; the term blowing would more easily be dismissed, and we might call the action *driving* it along.

The term sweeping is in some instances used to express the action of wind.

When we associate this agency of the wind with its effect on a field of ripening corn, we conceive of it *sweeping* along or over the corn; and the same effect would follow, were we to conceive of a large brush of long hair carried on the tops of corn; so that however popular expression, or the prejudice of custom may operate to distinguish the causes producing the same effect, it becomes us to waive such distinctions, and admit the efficiency of the one as much as the other.

Instances of the action of wind in a chimney.

I have observed that when a door has been suddenly opened or shut, it has brought down soot in a chimney which has a sluggish draft; or a piece of paper accidentally thrown on the fire has had the like effect; and this by a momentary acceleration of the current of air in the flue. What then will not such a machine as this perform, with such a volume of air, with such a velocity.

The air machine sweeps chimnies of all heights, and is durable.

One circumstance presents itself, by which the air machine must have a decided preference over the brushes; which is this, you must know the height of the chimney, and adapt the length of the rods to that height, or else you will not know when you are in the flue or out of it. Whereas my machine knows no necessity for such a punctilio; all that is necessary being only to give so many shocks of the condenser to a chimney of two stories, and so many to one of three, and so on. And again, these brushes in the very outset of trial made with them, are subject to accidents, and will most assuredly wear out very fast. What must become of those whalebone rods when the sewing is rubbed through in passing up and down against the projections of unceremonious bricks and mortar? What repairs will they not be subject to in the course of one day's action? Whereas the air machine will sweep a hundred chimnies, and be repaired for two-pence; it wanting only a little oil in the condenser.

It was tried at the Adelphi upon chimneys that contained no soot.

When this machine was carried to the Adelphi, it came to be the third in course for the trial, and it was ordered *down*, down to the foundation of the house, for fear a chimney should not be found high enough in any other part of the building.— But when it was brought, behold there had been two sweepers already, which obliged me to apologize for the machine, by observing

observing that they had taken down all the soot, and left nothing for it to do *. Upon which a suggestion was made, that there was another chimney which had a fire alternately during the winter †; and on examination it was agreed, that, by the appearance of the soot about the breast of the flue, the chimney was very foul, of which, however, I had my doubts. But it was thought expedient to try that chimney; and the first charge we gave it brought no soot. We gave it another charge, but no soot came; and it was thought best to send up a boy, with proper implements, to sweep the chimney; but no soot came down, except what may be said to be brought away by violence, and did not exceed a quart-potful, and with it much mortar or pargetting.

This brings me to some observations I have made on the condition of chimnies in general, as to the soot they may contain.

I tried this machine at Shoreditch Workhouse, where the chimnies are very lofty, for which reason I preferred them; but when I came to see their circumstances I suspected how it was, and we tried two of them where the fires had been very economical, and may continue so winter after winter, without ever requiring to be swept at all, for reasons I will give by and by. I then went to a neighbour of mine, and we swept two of his chimnies; one had no soot, the other had a good deal; and we thought fit to get a boy to go up the last chimney, and he brought down about a hatful of soot; however, my neighbour was satisfied, and signed my certificate to the Committee at the Adelphi.

We then went to another neighbour's, and swept his parlour chimney, and dislodged a great quantity of soot. We were to have swept his kitchen chimney, but it having a smoke-jack, we could not apply the machine for want of the additional pipe in *Fig. 2*.

* It appears the committee had no regard to the effect of any of the candidate's inventions, any further than a fair thrust up the chimney.

† With another in the same room I suppose.

IV.

Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double Stars; with an Investigation of the Cause to which they are owing. By WILLIAM HERSCHEL, LL. D. F. R. S. From the Philosophical Transactions for 1803.

(Concluded from Page 227.)

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

I SHALL now enter into a more detailed examination of the several angles of position I have taken at different times, and show that they agree perfectly well with the appearances which must arise from the revolution of a small star round Castor. A calculation of these angles may be had, by finding the annual motion of the small star, from the change of $21^{\circ} 54'$, which has been shown to have taken place in 23 years and 142 days. Accordingly, I have given, in the first column of the following table, the time when the angles were taken. In the second are the angles as they were found by measure; they are all in the north-preceding quadrant. The third column contains a calculation from the annual motion of $56', 18$, obtained as before mentioned: it shows what these angles should have been, according to our present supposition of a revolving star. And the last column gives the difference between the observed and calculated angles.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Nov. 5, 1779 -	$32^{\circ} 47'$	$32^{\circ} 47'$	$0^{\circ} 0'$
Feb. 23, 1791 -	22 57	22 11	+ 0 46
Feb. 26, 1792 -	27 16	21 16	+ 6 0
Dec. 15, 1795 -	13 52	17 42	- 3 50
March 26, 1800	18 8	13 41	+ 4 27
April 23, 1800	10 30	13 37	- 3 7
Dec. 31, 1801 -	7 58	12 2	- 4 4
Jan. 10, 1802 -	10 53	12 1	- 1 8
Jan. 23, 1802 -	10 28	11 59	- 1 31
Feb. 28, 1802 -	13 0	11 53	+ 1 7
Feb. 11, 1803 -	7 53	11 0	- 3 7
March 23, 1803	13 23	10 54	+ 2 29
March 27, 1803	10 53	10 53	0 0

On

Looking over the fourth column of this table, it will be observed that the differences between the observed and calculated are not greater than may be expected, considering that the early measures are single, and cannot have the accuracy which may be obtained by repetition. Even as they must acknowledge them sufficient to ascertain the change in the angle of position of the two stars. In one the difference amounts to six degrees; but it will soon be that a more accurate annual motion gives a calculated which takes off much of the error of this measure.

In conversation with my highly esteemed friend the Astronomer Royal, he happened some time ago accidentally to mention that Dr. Bradley had formerly observed the two stars of α Crum to stand in the same direction with Castor and

It occurred to me immediately, that if the time of this observation could be nearly ascertained, it would be of the importance to the subject at present under consideration, should Dr. Bradley's position be very different from the stated one, it would induce us at once to give up the idea of a moving star. The observation was made by Dr. Bradley a few years ago to see whether any change could be perceived in the position of the year, by which the annual parallax of the stars was discovered. Dr. Maskelyne, who had this information from Dr. Bradley in conversation, had made a memorandum in his papers. He has been so kind as to look for it; and soon as he found the note, he sent me the following which I have his permission to transcribe.

*able star Castor. No change of position in the two stars :
joining them, at all times of the year, parallel to the line
; Castor and Pollux in the heavens, seen by the naked*

Dr. Maskelyne informs me, that the observation must have been made about the year 1759; and also mentions, that he has verified the fact, as to the line joining the two stars as seen through the telescope parallel to the line joining Castor and Pollux, in 1760 or 1761; but that he did not examine it at any other times of the year.

The advantage of having an angle of position observed in 1759, Dr. Bradley, and so soon after verified by Dr. Maskelyne, will give us an addition of 20 years to our period. On comparing the right ascension and polar distance of Castor and Pollux

Observations and inferences respecting the changes of relative situation in stars extremely near each others

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

Pollux for November 5, 1759, it appears, that a line drawn from Pollux through Castor, must have made an angle of $56^{\circ} 32'$ north preceding with the parallel of that star; and, this being also the position of our double star, we have an interval of 43 years and 142 days, for a change of $45^{\circ} 39'$, from the time of Dr. Bradley's observation to that of my last measure of the angle. By this we are now enabled to correct our former calculation, which was founded upon a supposition that the first angle of position I had taken was perfect; but this could hardly be expected, and on examination it appears that the measure was $2^{\circ} 40'$ too little. The annual motion, by our increased period, is $1^{\circ} 3', 1$; and the computation of the angles of position in the third column of the following table, as well as the differences contained in the fourth, are made according to this motion.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Nov. 5, 1759 -	$56^{\circ} 32'$	$56^{\circ} 32'$	$0^{\circ} 0'$
Nov. 5, 1779 -	32 47	35 29	- 2 42
Feb. 23, 1791 -	22 57	23 36	- 0 39
Feb. 26, 1792 -	27 16	22 32	+ 4 44
Dec. 15, 1795 -	13 52	18 32	- 4 40
March 26, 1800	18 8	14 3	+ 4 5
April 23, 1800	10 30	13 58	- 3 28
Dec. 31, 1801 -	7 58	12 12	- 4 14
Jan. 10, 1802 -	10 53	12 10	- 1 17
Jan. 23, 1802 -	10 28	12 7	- 1 39
Feb. 28, 1802 -	13 0	12 1	+ 0 59
Feb. 11, 1803 -	7 53	11 1	- 3 8
March 23, 1803	13 23	10 54	+ 2 29
March 27, 1803	10 53	10 53	0 0

When the result of this table is compared with that of the former, it will be seen that my observations agree not only very well with Dr. Bradley's position, but even give more equally divided differences than before, so that the excess and differences counteract each other better than in the first table.

The time of a periodical revolution may now be calculated from the arch of $45^{\circ} 39'$, which has been described in 43 years and 142 days. The regularity of the motion gives us great reason to conclude, that the orbit in which the small star moves
about

about Castor, or rather, the orbits in which they both move round their common centre of gravity, are nearly circular, and at right angles to the line in which we see them. If this should be nearly true, it follows, that the time of a whole apparent revolution of the small star round Castor, will be about 342 years and two months.

Observations and inferences respecting the change of relative situation in stars extremely near each other.

γ Leonis.

Our foregoing discussions will greatly abridge the arguments which may be used, to shew that this star and its small companion are also probably united in forming a binary system. But, in order to give more clearness to our disquisition, we shall follow the arrangement which has been used with α Geminorum, and prefix the same letters to our paragraphs. Then, if any one article should appear to be not sufficiently explained, we need but turn back to our first double star, where the same letter will point out what has already been said more at large on the subject; and an application of it may easily be made.

The distance of the stars γ and x , as I shall again call the small one, has undergone a visible alteration in the last 21 years. The result of a great number of observations on the vacancy between the two stars, made with the magnifying powers of 278, 460, 657, 840, 932, 1504, 2010, 2589, 3168, 4294, 5489, and 6652, is, that with the standard power and aperture of the 7-feet telescope, the interval in 1782 was $\frac{1}{4}$ of a diameter of the small star, and is now $\frac{3}{4}$. With the same telescope, and a power of 2010, it was formerly $\frac{1}{2}$ of a diameter of the small star, and is now full 1 diameter. In the years 1795, 1796, and 1798, the interval was found to have gradually increased; and all observations conspire to prove, that the stars are now $\frac{1}{2}$ a diameter of the small one farther asunder than they were formerly. The proportion of the diameter of γ to that of x , I have, by many observations, estimated as 5 to 4.

The first measured angle in 1782, is $7^{\circ} 37'$ north following*; and the last, which has been lately taken, is $6^{\circ} 21'$ south fol-

* In my second Catalogue of double Stars, (Phil. Transf. for 1785, page 48,) the angle of position is $5^{\circ} 24'$. This was taken April 18, 1783; and, not being acquainted with the motion of the small star, I supposed it to be more accurate than the former measure.

Observations and inferences respecting the change of relative situation in stars extremely near each other.

lowing. The sum of these angles gives $13^{\circ} 58'$, for the change that has taken place in 21 years and 38 days. To account for this, we are to have recourse, as before, to the various motions of the three bodies.

Single Motions.

(a) The motion of x alone cannot be admitted, since it is known that γ Leonis is not at rest. The annual proper motion of this star, according to M. De la Lande, is $+0''.38$ in right ascension, and $0''.04$ in declination towards the south.

(b) γ cannot be the only moving body; because its motion in right ascension only, which, in 21,1 years, at the parallel of γ , amounts to $7''.49$, would have long ago taken it away from the small star.

(c, d, e,) The sun cannot be the only moving body; because its motion in right ascension will not account for that of γ Leonis, which star therefore cannot be at rest. And, if we were willing to give up the former assumed solar motion, in order to fix upon such a one as would explain the motion of γ , we should be under a necessity to contradict the united evidence of the proper motions of many principal stars which are in opposition to it.

Double Motions.

(f) When two motions are proposed, we cannot fix upon γ and x for the moving bodies, unless we should set aside the solar motion, and this, we know, cannot properly admit of a doubt.

(g) That we cannot allow O and x to be the two bodies in motion, follows from the insufficiency of the solar motion to account for that of γ , which must be real, or at least partly so.

(h) If O and γ are the moving bodies, the given situations of x , in the years 1782 and 1783, point out an apparent motion of x , which must be intirely owing to the solar parallax; and, therefore, those who will admit this hypothesis, must grant the discovery of the motion of the solar system, and of the proportional parallax of the two stars γ and x . Let us however examine whether any motion of the sun, such as we can admit, will account for the change of position and distance pointed out by my observations of the small star near γ Leonis.

The

The joint effect of proper motion and parallax, has carried γ from its situation in 1782 to that where we now find it. The small star, having all this time, in appearance, accompanied γ , must have gone through a space of $7''.98$, in a direction which makes an angle of $8^\circ 30'$ south following with the parallel of γ , in order to be at its present distance from it, and at the same time to have undergone the required change of its angle of position. Now, as the supposition we are examining requires this small star to be actually at rest, it will be necessary to assign to the sun an opposite motion of the same velocity, in order to make that of γ only an apparent one. The consequence of this will be a retrograde motion of the sun, which it is well known cannot be admitted.

Observations and inferences respecting the change of relative situation in stars extremely near each other.

Motion of the three Bodies.

(i) A motion of all the three bodies, is the only way left to explain the phenomena of our double star; and I shall now again point out the very particular circumstances which it is requisite should all happen together, to produce the intended effect.

Let the motion of the sun, with the same annual velocity 1, as in the case of α Geminorum, be directed towards λ Herculis. Then the effect of this motion will shew itself at the place of γ Leonis, in the annual velocity of .3314, and in a direction which makes an angle of $31^\circ 11'$ south preceding with the parallel of that star. In this calculation, I have admitted the distance of the largest of the two stars of γ from the sun to be 3, that of α Geminorum being 2. But, if any other distance should hereafter be considered as more probable, the calculation may be easily adapted to it. The consequence of the parallax thus produced on γ Leonis in 21,1 years, will be an apparent motion of $2''.788$ south preceding, in the abovementioned direction; and, on α , it will be in the same time, and in the same direction, $1''.091$. As the small star must not be too near γ , we have, in the calculation, supposed it to be at the distance of 4 from O.

The real annual proper motion of γ is required to be 3,5202; and its direction must make an angle of $3^\circ 40'$ north following with the parallel. By this motion alone, γ would have passed over a space of $9''.87$ in 21,1 years; but, when it is combined with the apparent motion arising from parallax, the star will come into its present situation.

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

The real annual motion of x must be 4,6294, in a direction $0^{\circ} 20'$ south following. This will carry it over $9''.74$, in 21,1 years; and, when combined with the apparent motion which the solar parallax will occasion, both together will bring it to its proper distance from γ Leonis, and to a situation which will agree with the last observed angle of position.

From what has been said, it is again evident, that not only as many particular circumstances must concur in explaining the phenomena of γ Leonis as we have pointed out with α Geminorum, but that a very marked condition is added in our second double star, which requires an adjustment of velocities in γ and x , which shall also fit the same solar motion that was used in α Geminorum. And this proves, that every additional double star which requires the same condition in order to have its appearances explained, will inforce the arguments which have been used, in a compound ratio.

If, on the other hand, we have recourse to the simplicity of the known effects of attraction, and admit the two stars of our present double star to be united in one system, all the foregoing difficulties of accounting for the observed phenomena will vanish. Whatever may be the proper motion of the sun, the parallax arising from that cause will affect both stars equally, on account of their equal distance from the sun. The proper motion of γ Leonis also may be in any direction, and of any given velocity, such as will agree best with astronomical observations; since the motion of a system of bodies will not interfere with the particular motion of the bodies that belong to it, so that our secondary star will continue its revolution round the primary one without disturbance.

It will now be necessary to examine the observed angles of position, and to compare them with calculated ones; but, as there has been a change in the distance of the two stars, it is evident that, if they revolve in circular orbits, the situation of the plane of their revolution must be considerably inclined to the line in which we see the principal star.

Let N F S P, Fig. 2, be the orbit in which x revolves about γ placed in the centre. Suppose a perpendicular to be erected at γ leading to O, not expressed in the figure. By an observation of Feb. 16, 1782, we have the angle $F \gamma x = 7^{\circ} 37'$ north following; and the proportion of the apparent diameter of γ to that of x has been given as 5 to 4. It has also been ascer-

tained

tained, that the vacancy between the apparent diameters, when the first angle of position was taken, was $\frac{1}{4}$ diameter of the small star; and the last angle of position being $6^{\circ} 21'$ fourth following, with a distance between the stars of $\frac{1}{4}$ diameter of the small star, we obtain the two points or centres of the small stars xx' , through which an ellipsis $abxx'cd$ may be drawn about γ . This will be the apparent orbit in which the small star will be seen to move about γ , by an eye placed at O . And the inclination of the orbit to the line in which we see the double star, will be had sufficiently accurate to enable us to give a calculation of the several angles of position that have been taken. The ellipsis we have delineated shews that the small star, in its first situation x , could not be much past its conjunction at b , and that, consequently, in passing from x to x' , the parts of the apparent elliptical arch, which are projections of the real circular arch $h h'$, would be described in times nearly proportional to the time in which the whole arch has been described. Upon these principles, the third column of the following table has been calculated.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Feb. 16, 1782 -	$7^{\circ} 37' nf$	$7^{\circ} 37'$	$0^{\circ} 0'$
April 18, 1783 -	$5 24 nf$	$6 51$	$- 1 27$
Jan. 24, 1800 -	$3 16 sf$	$4 15$	$- 0 59$
Feb. 19, 1800 -	$3 23 sf$	$4 18$	$- 0 55$
March 26, 1800	$3 47 sf$	$4 22$	$- 0 35$
Jan. 26, 1802 -	$6 4 sf$	$5 35$	$+ 0 29$
Feb. 10, 1803 -	$3 33 sf$	$6 16$	$- 2 43$
March 22, 1803	$6 32 sf$	$6 20$	$+ 0 12$
March 26, 1803	$6 21 sf$	$6 21$	$0 0$

The difference between the calculated and observed angles, contained in the fourth column of the preceding table, is so little, that we may look upon the gradual change of these angles as established by observation; and we may form a calculated estimate of the time which will be taken up by the mutual revolution of the two stars. The apparent places xx' , being referred to their real ones, give the arch $h h'$, which has been described in 21 years and 38 days; and this arch, seen from the centre γ , is about $6^{\circ} 20'$: it follows, that the length of a whole revolution of our small star round γ Leonis, will be about 1200 years.

♌ Bootis.

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

This beautiful double star, on account of the different colours of the stars of which it is composed, has much the appearance of a planet and its satellite, both shining with innate but differently coloured light.

There has been a very gradual change in the distance of the two stars; and the result of more than 120 observations, with different powers, is, that with the standard magnifier, 460, and the aperture of 6,3 inches, the vacancy between the two stars, in the year 1781, was $1\frac{1}{2}$ diameter of the large star, and that it now is $1\frac{1}{4}$. By some earlier observations, the vacancy was found to be considerably less in 1779 and 1780; but the 7-feet mirror then in use was not so perfect as it should have been, for the purpose of such delicate observations. By many estimations of the apparent size of the stars, I have fixed the proportion of the diameter of ι to that of α , as 3 to 2. August 31, 1780, the first angle of position measured $32^{\circ} 19'$ north preceding*; and, March 16, 1803, I found it $44^{\circ} 52'$, also north preceding; the motion, therefore, in 22 years and 207 days, is $12^{\circ} 33'$. It should also be noticed, that while the apparent motion of α Geminorum, and of γ Leonis, is retrograde, that of ι Bootis is direct.

A proper motion in this star, if it has any, is still unknown; our former arguments, therefore, cannot be applied to it, without some additional considerations; and, as many others of my double stars will stand in the same predicament, I shall give an outline of what may be said, to show that this, and probably many of the rest, are also binary systems.

Single Motions.

(a—c) If ι Bootis is a star in which no proper motion can be perceived, we may infer, from the highly probable motion of the solar system, that this star, which is of the third magnitude, and on that account within the reach of parallax, must have a real motion, to keep up with the sun, in order to prevent an

* The angle of position, in my first Catalogue of double Stars, Phil. Transf. for 1782, page 115, is $31^{\circ} 34'$ (it should be 54°) north preceding. This will be found to be a mean of the three first measures hereafter given in a table of positions.

apparent

apparent change of place, which must otherwise have happened. In this case, no single motion can be admitted to explain the phenomena of our double star. But, if a real proper motion of ϵ Bootis should hereafter be ascertained, the arguments we have used, in the case of γ Leonis, will lead to the same conclusion.

Observations and inferences respecting the change of relative situation in stars extremely near each other.

Double Motions.

(f) ϵ and x cannot be the moving bodies; and our former argument (f) will apply to every double star whatsoever.

(g) O and x cannot be alone in motion; for, if no motion in ϵ can be perceived, it must move in a similar manner with the sun, and none of the three bodies will be at rest. But, if its proper motion shall hereafter be found out, it must either be exactly the reverse of the solar motion, and therefore only an apparent one, or it will be more or less different. In the latter case, all the three bodies must be in motion; in the former, the exact quantity of the solar motion will be discovered, and the relative parallax of many stars may be had by observation.

(h) If O and ϵ are the two bodies in motion, and if at the same time no motion in ϵ can be perceived, then the apparent motion of x must be intirely owing to the different effect of the solar parallax on ϵ and x ; but the effect of the solar parallax on x , can only be in a direction contrary to the motion of the sun, which, being north following the small star, whether it be nearer or farther from us than ϵ , must have an apparent motion towards the south preceding part of the heavens. But this is directly in opposition to my observation of the motion of the small star, which, these last 23 years, has been directed towards the north following.

Motion of the three Bodies.

(i) Let the motion of the sun be again towards λ Herculis; then, if no motion in ϵ Bootis be perceivable, it must move exactly like O. Highly improbable as it is, let it be admitted.

Then, in addition to this extraordinary supposition, a third motion is also required for x , which, aided by the solar parallax, is to carry it likewise within a quarter of a diameter of ϵ , into the same place where, though unperceived, the large star has been carried by its own motion; that is, in order to be apparently

Observations and inferences respecting the change of relative situation in stars extremely near each other, rently at rest, the sun, ϵ Bootis, and its small companion, must all move exactly alike, setting aside the very little difference in the position and distance of the small star, which, in the whole, amounts to little more than 6-tenths of a second; than which, certainly nothing can be more improbable.

But, if ϵ shall hereafter be found not to have been at rest during the time of my observations upon it, then its place will be given; and, since also the situation of x , with respect to ϵ , is to be had from my angles of position and distances of the two stars, the case will be similar to that which has already been considered, in the paragraph (i), under the head of γ Leonis.

I may here add a remark with regard to ϵ Bootis, which will be applicable to several more of my double stars. In the milky-way, a multitude of small stars are profusely scattered, and their arrangement is very different from what we perceive in those parts of the heavens which are at a considerable distance from it. About ϵ Bootis, which is situated in what I have formerly called figuratively a nebulous part of the heavens*, there are, comparatively speaking, hardly any stars; and, that so remarkable a star as ϵ should have a companion, seems almost to amount to a proof that this very companion is, as it appears to be, a connected star. The *onus probandi*, therefore, ought in justice to fall to the share of those who would deny the truth of what we may call a fact; and I believe the utmost they could do, would be to prove that we may be deceived; but they cannot show that this star has no connection with ϵ Bootis.

This argument will be much supported, when we consider that many of the double stars in the milky-way are probably such as have one of the scattered stars, nearly in the same line, at a great distance behind them. In this case, the two stars of the double star have no connection with each other; and the great number of them in the milky-way, is itself an indication of this effect of the scattered multitude of small stars. In the single constellation of Orion, for instance, we have no less than 43, pointed out by my catalogues; ten of which are of the first class, and yet have undergone no change of distance or position since I first perceived them. But, with apparently insulated stars, such as ϵ Bootis, the case is just the reverse.

* See Phil. Transf. for 1784, page 449.

If, in consequence of our former arguments, and the present Observations and remarks, we place ϵ Bootis among the stars which hold a smaller inferences respecting the one in combination, we may delineate its orbit as in Plate VIII. change of relative situation in stars extremely near each other. Fig. 3.

Let P N F S represent a circle, projected into the elliptical orbit $axx'bcd$. ϵ is the large star; and xx' are the first and last measured north preceding situations of the small one, as given in the following table.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
August 31, 1780	32° 19'	33° 58'	- 1° 39'
March 13, 1781	30 21	34 13	- 3 52
May 10, 1781 -	33 1	34 18	- 1 17
Feb. 17, 1782 -	38 26	34 40	+ 3 46
August 18, 1796	45 32	41 40	+ 3 52
Jan. 28, 1802 -	49 18	44 19	+ 4 59
August 31, 1802	46 47	44 36	+ 2 11
March 23, 1803	43 43	44 52	- 1 9
March 26, 1803	44 52	44 52	0 0

The real motion from h to h' is projected into that from x to x' ; and, while the elliptical arch subtends an angle of $12^\circ 33'$, the circular one will be about $4^\circ 50'$.

From the figure of the orbit, we may conclude that the small star, in its first position, at x or h , was not more than between 30 and 40 years past its conjunction; and that, consequently, the parts of the arch xx' , were nearly proportional to the times of their being described. The positions have been calculated upon this principle; but with some allowance for the first observed angle, which I suppose to have been a little too small; and, though the differences of the observed and calculated angles are pretty considerable, the observations are still sufficiently consistent to prove the gradual change of the situation of the small star.

The quantity of the change in 22 years and 207 days, will show that a periodical revolution cannot take up less than 1681 years. The real figure and situation of the orbit, with many other particulars, are still unknown; it is, therefore, unnecessary to point out the uncertainties in which the investigation of the periodical time of the small star about ϵ Bootis must long remain involved.

ζ Herculis.

ζ Herculis.

Observations
and inferences
respecting the
changes of rela-
tive situation in
stars extremely
near each other.

My observations of this star furnish us with a phenomenon which is new in astronomy; it is, the occultation of one by another. This epoch, whatever be the cause of it, is equally remarkable, whether owing to solar parallax, proper motion, or motion in an orbit whose plane is nearly coincident with the visual ray. My first view of this star, as being near the pole, was July 18, 1782. With 460, the stars were then $\frac{1}{2}$ of the small star asunder. The large star is of a bluish white; and the small one ash-coloured.

July 21, of the same year, I measured the angle of position $20^{\circ} 42'$ north following. With the standard power, the distance of the stars remained as before. With 987, the stars were just full diameter of the small one asunder.

In the year 1795, I found it difficult to perceive the small star; however, in October of the same year, I saw it double, with 460; and its position was north following.

Other business prevented my attending to this star in the year 1802, when I could no longer perceive the small star. Sometimes, however, I suspected it to be still partly visible, and, in September of the same year, with 460, the night being very clear, the apparent disk of *ζ Herculis* seemed to be a little lengthened one way. With the ten-feet telescope, of power of 600, I saw the two stars of *α Coronæ* very distinctly, and, having in this manner proved the instrument to be good, I directed it to *ζ Herculis*, and found it to have the appearance of a lengthened, or rather wedge-formed star; after which I took a measure of the position of the wedge.

Our temperature is seldom uniform enough to permit the use of very high powers; however, on the 11th of April, 1803, I examined the apparent disk, with a magnifier of 2140, and found it, as before, a little distorted; but there could not be more than about $\frac{1}{3}$ of the apparent diameter of the small star wanting for a complete occultation. Most probably, the path of the small star was not quite central; if so, the disk will remain a little distorted during the whole time of the conjunction. Our present observations cannot determine which of the stars is at the greater distance; but this will occasion no difference in the appearance, for, if the small star should be the nearest, its light would be equally lost in the brightness of the large one.

The observations I have made on this star, are not sufficient to direct us in the investigation of the nature of the motion by which this change is occasioned.

We may however be certain, that with regard to

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

Single Motions.

(*a, b*) Neither x nor ζ can be supposed to be the only moving bodies, without contradicting the highly probable arguments for the sun's motion.

(*c, d*) If we admit the sun to be the moving body, the stars ζ and x being at rest, we may calculate the effect of the solar parallax upon them, as follows. Let O move towards α Herculis, with the annual velocity 1, as in the case of α Geminorum; then, from the situation and magnitude of the large star of ζ Herculis, which we will suppose 4 m , the effect of the solar motion at ζ will be only ,0522; and, at x , supposed to be at the distance 5 m , it will be ,0418. This will show itself at the parallel of ζ in a direction of $25^{\circ} 5'$ north preceding, the solar motion being in the opposite direction south following. But this parallax will only produce, in 20 years and 10 months, an apparent change of $0''.444$ in ζ , and of $0''.355$ in x ; and will separate the stars, instead of bringing them to a conjunction.

(*e*) A considerable advantage may be gained, by placing x at a little more than $\frac{2}{3}$ the distance of ζ from O . For as, in the above-mentioned time, this would make the effect of parallax upon it $1''.18$, a conjunction should now take place. But then the stars, though very near each other, would not be quite in contact; much less could one of them occasion an occultation of the other. The supposition also, that the small star should be only $\frac{2}{3}$ of the distance of the large one from us, is not very favourable to the hypothesis.

δ Serpentis.

This double star has undergone a very considerable change in the angle of position, but none in the distance of the two stars. The 5th of September, 1782, an accurate measure of the position was $42^{\circ} 48'$ south preceding; and February 7, 1802, it measured $61^{\circ} 27'$ south preceding. In 19 years and 155 days, therefore, the small star has moved, in a retrograde order, over an arch of $18^{\circ} 39'$.

Every

In 1795 or 1796. Mr. Mc Kean communicated the idea to me a few days after it first occurred to him, in the year 1795 or 1796; his plan differed, however, in two points, from that described in your Journal; for, firstly, he made use of handspikes applied into holes in the end of the largest barrel, whenever the winch did not afford him sufficient power; and, secondly, when he found it necessary to raise any thing above the level of his windlafs, he added two other pullies for the rope to pass over,

Dimensions and application of a compound barrel.

About the year 1797, Mr. Mc Kean erected a saw-mill worked by a steam engine, at Bordenton in New Jersey; where he employed this crane to raise his logs, from the ground to the frame on which they were to be cut, being a height of about twelve feet; the circumference of the largest barrel was about six feet, and its length three feet; the smallest barrel was of the same length, and its circumference three feet, making the difference of their circumferences three feet; and, consequently one turn of the barrels, raised the log one foot six inches; and eight turns only were sufficient, to elevate it the height required.

Its power as one to twenty.

This windlass was turned with handspikes six feet long; which, at the place where the power is generally applied, would describe a circle of about 30 feet; so that the power obtained, is as one to twenty. The length of rope necessary, including that which passes round the pullies, and from thence to the barrels, is about 55 feet.

A common tackle would have required five times as much rope; but thinner.

Now in order to raise a weight 12 feet high, with a power of one to twenty, by means of the common block pullies, no less than 270 feet of rope is necessary; there is, however, this difference in the sizes of the ropes, that, in the former case supporting one half of the weight to be raised, whereas in the latter it has only to sustain one twentieth part of it.

This windlass does not run down.

The windlass has the peculiar property of holding the weight at any part of its rise or fall, without needing a ratchet-wheel and catch; by reason of the two parts of the rope pulling on opposite sides of the barrels; and although one pulls on a longer lever than the other, yet it is not sufficient to overcome the friction on the axis, which must of necessity be large, in order to give the requisite strength to the machine.

It promises to be extensively useful.

I have lately introduced a small windlass of this kind, into a piece of machinery requiring alternate elevation and depression,

pression, and find it extremely convenient; and I have no doubt it will be adopted in a great variety of machinery when its valuable properties are generally known.

I am,

Sir, Yours, &c.

J. J. HAWKINS.

11, New Life Street,

March 17, 1804.

VI.

Proposal for constructing a Galvanic Apparatus of Great Power, for the Combustion of Metals. By a Correspondent. I. R. I.

To Mr. NICHOLSON.

SIR,

THE following idea of a mode of increasing to an immense degree the galvanic power of inflaming metals, took its origin from Mr. Wilkinfon's paper on that subject in your last Journal; and I beg that if you think this communication can be of the smallest use to that ingenious gentleman, that you will send it to him before publication.

I can hardly think that it could have escaped his penetration, (though I cannot account for his omitting the observation) that the natural consequence of his discovery relative to the ratio by which the galvanic trough increases in its power of inflaming metals, is, that the maximum of effect will be produced (from a given surface,) by one pair only; so that the same surface of metal that in his apparatus at present constructing and distributed into 50 pairs, will burn 108 feet of steel wire $\frac{7}{16}$ of an inch thick, if concentrated into one, will have the same effect on 5400 feet of the same wire; an enormous increase of a power that may become as noble a present to the arts, from science, as the steam-engine has proved.

I have also turned my thoughts towards making a new form of apparatus, on the above principle, so as to condense it into as little bulk as possible. The simple inspection of the drawing accompanying this will sufficiently illustrate it; the scale is of three inches to a foot, (Pl. XV.) it represents part of the ground plan of a trough, of about ten feet long, two feet broad, and two and half feet deep: the black line represents the copper, the dotted line the

Increase of galvanic combustion.

by one single plate or pair.

Description of the proposed apparatus.

the zinc, the white the acid liquor, and the shaded part the wood: the scale is made large for the sake of distinctness; two spigots or turncocks are adapted to the end by which the liquor may be drawn off; a contrivance of great use, but impracticable in the present form; only two convolutions of the plates are expressed, and the turns are made about three inches wide, to facilitate the folding of the zinc plates to the copper; the liquor is confined in as small a space as possible, to prevent the unnecessary waste of acid. It may also be constructed with one metal only, on Mr. Davy's plan; in that case, the sulphuret will occupy one side of the plate, and the acid the other. Hoping that the above may merit a place in your very excellent Journal, and be of use to my fellow labourers in science,

I remain, Sir,

Your most obedient humble Servant,

I. R. I. an old Correspondent.

Edinburgh, March 13, 1801.

VII.

A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake and other American Serpents. By BENJAMIN SMITH BARTON, M.D. From the American Transactions, Vol. IV.*

FIDEM NON ABSTULIT ERROR.

Naturalists frequently too credulous.

NATURALISTS have not always been philosophers. The slight and superficial manner in which they have examined

* Since this memoir was read before the Society, it has been considerably altered, and somewhat enlarged. I hope, the alterations will render it more worthy of the notice of those who, like myself, derive pleasure and happiness from the contemplation of the works and operations of nature, on this globe.

I fear, I shall be thought to have treated the question in too diffusive a manner. I have not, indeed, laboured to be concise. But if the memoir is more extensive than was necessary, I flatter myself, it will be admitted that it, at least, contains some new and interesting facts. I submit it to its fate. B. S. B.

This interesting paper has been deferred from the pressure of other matter of more immediate and temporary claim. W. N.

many

the subjects of their science; the credulity which has misled them in their researches after truth, and the prejudice with which they have decided upon many questions of importance, are proofs of this assertion.

It is a question in natural history that has, in an especial manner, solicited from me these observations. I mean the question concerning the fascinating faculty, which has been applied to different kinds of American serpents. It is my intention to examine this question, in the memoir which I now present to the Philosophical Society.

By this fascinating faculty we have all heard and read. In our country situations, there is hardly a man or a woman who will not, when the subject comes to be mentioned, seriously relate some wonderful story, as a confirmation of the doctrine. Children seem taught to believe it. It is sometimes one of the earliest prejudices implanted on their tender minds. It is a prejudice which often increases with their years; and even in that happy period of life when the mind is most firm, and the least propense to the belief of extraordinary things, the ways of which we are not accustomed to scanning, I have known this prejudice so deeply and powerfully rooted, as to mock the light and sureness of all the strength of reasoning.

It is not my intention, in this memoir, to give an analysis, or a general view, of every thing that has been written on the subject. Examination of the question proposed.

Nor is it my intention to examine the many stories, told by authors, in support of the fascinating faculty of serpents.

For the first task, I have not leisure; and, as to the second, I should think my time ill employed in pointing out gross absurdities which seem to constitute a necessary part of many of those stories. I think it proper, however, to state that I have anxiously sought for, and have patiently perused the volumes of tales published in favour of the fascinating faculty, which I mean to controvert.

At giving a general, though correct, view of the subject, uninfluenced by the bold assertions of ignorance, or plausible conjectures of science. In the investigation of the question, I have sought for facts: these have been collected. I have studiously endeavoured to follow where the facts seemed to lead. Perhaps, they have led me astray.

The

Description of
the supposed
process of snakes
in fascinating
other animals.

The manner in which the supposed fascinating power of serpents is exerted has often been related, by different writers. I shall endeavour to convey some idea of the business, in few words as I can.

The snake, whatever its species may be, lying at the base of the tree or bush upon which the bird or squirrel sits, fixes its eyes upon the animal which it designs to fascinate or enchant. No sooner is this done than the unhappy animal, for the present, the language of those who differ from me in opinion, on this subject is unable to make its escape. It now begins to utter a most piteous cry, which is well known by those who hear it, and understand the whole machine of the business, to be the cry of a creature enchanted. If it be a squirrel, it runs up the tree for a short distance, comes down again, then runs up, and, lastly, comes lower down. "On that occasion," says an honest but rather credulous writer, "it has been observed, that the squirrel always goes down more than it goes up. The snake still continues at the root of the tree, with its eyes fixed on the squirrel, with which its attention is so entirely taken up, that a person accidentally approaching, may make a considerable noise, without the snake's so much as turning about. The squirrel as before mentioned comes always lower, and at last leaps down into the snake, whose mouth is already wide open for its reception. The poor little animal then with a piteous cry runs into the snake's jaws, and is swallowed at once, if it be not too small; but if its size will not allow it to be swallowed at once the snake licks it several times with its tongue, and smooths it, and by that means makes it fit for swallowing †."

Other accounts
are not materially
different.

It would be easy to cite, from different authors, other accounts of the manner in which the enchantment is performed, or, more properly speaking, of the conduct, or behaviour of the enchanting and enchanted animals. But between these accounts, there is hardly a specific difference. There is a considerable unity in all the relations that I have heard of,

* Professor Peter Kalm.

† Travels into North-America; containing its natural history and a circumstantial account of its plantations and agriculture, general, &c. &c. vol. i. p. 317 & 318. Also vol. ii. p. 207 & 209 & 210. English Translation. London: 1770 & 1771.

Hov

However, those who wish to examine this part of the subject more fully, will, at least, receive some degree of entertainment from the perusal of the many authors who have believed and asserted, that serpents possess a power of fascinating other animals.

That the belief in the existence of this power should have been so general among the uninformed part of a people, ought not to be wondered at. The human mind, unenlightened by science, or by considerable reflection, is a soil rich in the weeds of superstition, and credulity. It is ever prone to believe in the wonderful, even when this belief, as is often the case, brings with it fears, and cares, and misery. The bondage of the mind in superstitious credulity is great and heavy. Neither religion nor virtue can give it its freedom. This it obtains from science. How important, then, even in this point of view, is the enlargement of the mind by science!

Credulity in the vulgar is to be expected;

But it is, surely, a matter of some astonishment, that this belief should have been admitted, in all the fulness of its extravagance, by men of learning, of observation, and of genius: by those who have the book of nature in their hands; that book which will, in some future and some happier age, eradicate many of the prejudices which disfigure, and which mock the dignity of human nature: by classical scholars, grown old in the disbelief of similar fables, heightened and embellished by the charms of poetry; and also by the infidel, who denies the authenticity of scripture-miracles, few of which, even though they were not shown to be truths, are more improbable than the imaginary fact which I am examining.

but it is strange that men of ability have also admitted these facts.

I have sought to discover the original, or source of this belief. I do not find any traces of it among the ancient writers of either Greece or Rome. I think, it is most likely that no such traces can be found. Lucan, had serpents been thought to possess a fascinating faculty in his age, and in the country in which he lived, would, probably, have availed himself of its existence, in his beautiful account of the march of Cato's army through the Libyan-Desert*; and had such a notion prevailed in the earlier days of Læretius, would we not find some mention made of it in the poem *De Rerum Natura*, one of the finest and most varied productions of the human mind? Classical scholars may possibly, however, discover the dawn

This notion is not found in the writings of the ancients.

* Pharsalia, lib. IX.

of this notion in Greek and Roman authors, unread by me. On this subject, I have not pushed my inquiries as far as I wished to have done. It is not unlikely that I may examine the question more curiously, at some future period.

It is probable that in the mythology of Asia and of Africa, we shall discover some traces of this notion, so intimately connected with the superstitious credulity of a people, and even so naturally arising out of an imperfect view of the manners of serpents.

It does not appear to have originated nor to be much, if at all, admitted among the Indians.

If we believe the Reverend Dr. Cotton Mather *, Mr. Dudley †, and other persons, who had resided in North-America, we are to look for the beginning of this ridiculous notion among our Indians. How far, however, this is really the case may, I think, be doubted. It is certain that, at present, the opinion is by no means universal among the Indians. Several intelligent gentlemen, who are well acquainted with the manners, with the religious opinions, and with the innumerable superstitious prejudices of the Indians, have informed me, that they do not think these people believe in the notion in question. My friend Mr. John Heckewelder, of Bethlehem, writes to me, that he does not recollect to have heard the Indians say that snakes charm birds; though he has frequently heard them speak of the ingenuity of these reptiles in catching birds, squirrels, &c. Mr. William Bartram says, that he never understood that the nations of Indians among whom he travelled had any idea of the fascinating power of snakes ‡. On the other hand, however, a Mohegan-Indian told me that the Indians are of opinion that the rattle-snake can charm, or bewitch, squirrels and birds, and that it does this with its rattle, which it shakes, thereby inviting the animals to descend from the trees, after which they are easily caught. According to this Indian, his countrymen do not think that the snake, in any manner, accomplishes the business with its eyes. A Choktah-Indian assured me that the rattle-snake does charm birds, &c. but he was honest enough to confess that he

* The Philosophical Transactions, abridged, vol. v. part ii. no. 339. p. 162.

† Ibid: vol. vi. part iii. no. 376. p. 45.

‡ MS. note, communicated to me by this ingenious gentleman.

did not know in what manner it does it. The interpreter, through whom I conversed with this Indian, said that the snake charms by means of its rattle.

The veneration, or regard, which has been paid to the rattle-snake by certain North-American tribes seems, at first sight, to favour the opinion, that these tribes attributed to this hideous reptile some hidden power *, perhaps that of fascinating animals. Mr. William Bartram informs me, that the southern Indians, with whom he is acquainted, seem to hold the rattle-snake in a degree of veneration †. Mr. Heckewelder says that, to his certain knowledge, this reptile was once held in particular esteem by the Delawares. He was several times prevented, by these Indians, from killing the rattle-snake, being told that it was their grand-father, and, therefore, must not be hurt. At other times, he was told, he must not kill this snake, because the whole race of rattle-snakes would grow angry, and give orders to bite every Indian that might come in their way ‡. But, of late, especially among those Indians who have had connection with the whites, these ridiculous notions have mouldered away, and our Indians, at present, kill their rattling "grandfather" with as little ceremony as the Eskimaux are said to kill their parents in old-age.

Some North-American tribes had a veneration for the rattle-snake ;

It is obvious, from contemplating the manners and the history of nations, that a part of their religions, and a large part of the fabrick of their superstitious notions, have arisen

which seems to have been part of the religious system grounded on fear.

* Vis abdita. Lucretius.

† MS. note communicated to me.

‡ In my *Historical and Philosophical Inquiry* (not yet published), I have collected many facts which seem incontestibly to prove, that the mythology, or superstitious religion, of the Americans is a fragment of that mythology whose range in Asia, and in Africa, has been so extensive. Possibly, the veneration, or regard, which was paid to different kinds of serpents in America did not originate in this continent, but had its source in Asia, from which portion of the globe (after a long and laborious attention to the subject) I cannot doubt, that almost all the nations of America are derived. It is unnecessary, in this place, to cite instances of the religious veneration which was, and still is, paid to some species of serpents, in various parts of the old-world. These instances must be familiar to every person, who is acquainted with the historians or with the poets of antiquity, and with the history of the Gentoo-Indies.

out of fear. Perhaps, all mankind * admit the existence of two great beings, the one good and all-benevolent, the other bad and studious of evil. In our own continent, where, I believe, this notion was universal, certain tribes were assiduous in their adoration of the latter being, whilst the former, whom the light of reason taught them to consider as the source of life, and all their blessings, was merely acknowledged and named, but unworshipped and neglected †. The Delawares, and some other nations who speak dialects of their language, believe that a turtle, of an enormous size, inhabits the deep, and supports upon his back this continent, or, as they call it, island. They say it is in the power of this animal, by diving, to drown the world, as he has already done, in former ages. They, therefore, endeavour to conciliate his friendship and good-will. With this view, they make rattles of the turtle-shell, into which they put small stones, beans, or Indian-corn ‡, and play with this instrument, at their dances. The turtle is greatly esteemed by them; and, in the fulness of a mixed zeal and fear, they even deign to call him *Mannitto*, or God; because, they say, he can live both upon the land and in the water §.

It is not likely that they venerated the snake from a belief of its fascination.

It seems very probable to me, that the veneration for the rattle-snake had its birth in fear, and not in the belief that this reptile possessed the power of fascinating animals. If, as some writers have asserted, the Indians were in possession of absolute specifics for the bite of the rattle-snake, I am of opinion that the veneration for this animal would not have existed; or, at least, that it would not long have continued. But the Indians are often unable to prevent or to cure the effects of the active poison of this serpent, which not unfrequently destroys them ||.

I return

* I speak of mankind in the aggregate, and not of individuals among them.

† John De Laet, speaking of the Indians of New-York, has the following words: "Cæterum nullus ipsis religionis sensus, nulla Dei veneratio: diabolus quidem colunt sed non tam solemniter neque certis ceremoniis, ut Africani faciunt," &c. *Novus Orbis seu Descriptionis Indæ Occidentalis Libri xviii. lib. iii. cap. xii. p. 75. Lugd. Batav. 1633.*

‡ Maize.

§ MS. by Mr. John Heckewelder, *penes me.*

|| Adair says, he does "not remember to have seen or heard of an

I return to the more immediate path of my subject.

Among the Indians of South-America, I do not find any traces of the notion that serpents can fascinate other animals. Pifo, the author of the *Natural and Medical History of the two Indies*, seems to have been studious to bring together the extraordinary things which have been related of the rattle-snake. But he says not a syllable concerning the fascinating faculty of this reptile *.

The South Americans do not possess this notion,

But whatever may have been the native country of the notion which I am considering, it would have been well had it been confined to savages. It is a tale which seems nicely adapted to the wit and society of rude and uncultivated nations. Unfortunately, the progress of error and credulity is extremely rapid. Their dominion is extensive. The belief in the fascinating faculty of serpents has spread through almost all the

but it has spread over all America and Europe.

Indian dying by the bite of a snake, when out at war, or hunting; although they are then often bitten by the most dangerous snakes." *The History of the American Indians*, &c. p. 235. London: 1775. It is certain, from the testimony of many persons, that the bite of the rattle-snake has often proved mortal to the Indians, and others, notwithstanding the boasted specifics of these people. Father Cajetan Cattaneo says, many Indians die with the bite of serpents. "But," observes the father, "it is said they commonly escape with life, when they can quickly apply the remedy which providence has prepared of certain herbs, especially the spikenard, which some parts of Paraguay produce in plenty. But when they are bit by the rattle-snake it is confidently assured that the case admits no cure." *The third letter of F. Cajetan Cattaneo*. See *A Relation of the missions of Paraguay*, wrote originally in Italian, by Mr. Muratori. English Translation. p. 260. London: 1759. Father Cattaneo is here speaking of the South-American rattle-snake, the poison of which, I have little doubt, is more deleterious than that of the same animal in our part of North-America. Still, however, I am confident, that this poison, even in the most fervid climates, is not always mortal.

* Gulielmi Pisonis medici Amstelædamensis de Indiæ utriusque re naturali et medica libri quatuordecim. Amstelædami: apud Elzevirios, 1658. Some of Pifo's assertions concerning the rattle-snake are very extravagant. Such are the following: "Caudæ extremitate in anum hominis immissa, mortem infert confestim; venenum autem quod ore vel dentibus infundit, multo lentius vitam tollit." p. 275.

civilized

civilized parts of North-America. Nor is it confined to America. It has made its way into Europe, and has there taken possession of the minds of scholars, of naturalists, and of philosophers.

It is said that Dr. Johnson believed it. But, he was credulous, timid, and melancholy.

I think, I have somewhere either heard or read that the tale was credited by the late Dr. Samuel Johnson. If I am mistaken, I hope the admirers of this great man, should any of them read my memoir, will pardon me. It is certain, notwithstanding the vast strength and the rich fertility of Johnson's mind, that he was credulous and timid. Did this union of credulity and timidity arise out of that unhappy melancholy ("those casual eclipses which darken learning";) that often overclouded the brightness of the mind*? We are told that the Hercules of English literature believed in ghosts, and in the second-sight. The man who would thus suffer his mind to be estranged from probability, and entangled in difficulties, would, perhaps, find it easy to bend to the belief, that serpents have the faculty of fascinating other animals.

Linnaeus admits it.

Although I profess myself to be a warm admirer of Linnaeus, and although, at a very early period of my life, I enlisted myself under the banner of his school, I shall not, nevertheless, attempt to conceal, that this great man gave credit to the tale of the fascination of birds and other animals by serpents. In his *Systema Naturæ* (that immortal work), under the article *Crotalus horridus*, or the rattle-snake, he has the following words: "*Aves Sciurosque ex arboribus in fauces revocat.*"†. In another work, he speaks as follows. "Whoever is wounded by the Hooded Serpent (*Coluber Naja*) expires in a few minutes; nor can he escape with life who is bitten by the rattle-snake, (*Crotalus horridus*) in any part near a great vein. But the merciful God has distinguished these pests by peculiar

* Or, did his melancholy grow out of his credulity and fear?

† See volume first, p. 372. Vienna edition of 1767. Professor Gmelin, in his edition of the *Systema Naturæ*, when speaking of the rattle-snake, has the following words, viz. "*aves sciurique ex arboribus non raro in fauces inbiantis apertas incidunt,*" tom. i. pars iii. p. 1080. The same laborious author speaking of our grey-squirrel (*Sciurus cinereus*) says, "*a crotalo comeditur,*" tom. i. p. 147. This is true: but he might have said the same when speaking of the striped-dormouse, or ground-squirrel (*Sciurus striatus*), of our rabbit (*Lepus americanus*), and many other animals.

figns.

signs, and has created them most inveterate enemies; for as he has appointed cats to destroy mice, so has he provided the Ichneumon (*Viverra Ichneumon*) against the former serpent, and the Hog to persecute the latter. He has moreover given the *Crotalus* a very slow motion, and has annexed a kind of rattle to its tale, by the motion of which it gives notice of its approach: but, lest this slowness should be too great a disadvantage to the animal itself, he has favoured it with a certain power of fascinating squirrels from high trees, and birds from the air into its throat, in the same manner as flies are precipitated into the jaws of the lazy toad."*

His account.

Linnæus was, certainly, extremely credulous, though I do not find that any of his professed biographers have taken notice of this feature of his mind. But the proofs of my observation are numerous: they are to be found in almost every essay that he has written. His credulity with respect to the powers of medicines is, perhaps, peculiarly striking †. How far this credulity, in a mind otherwise truly great (a mind which with respect to the arrangement of natural bodies has never been equalled), is to be sought for in the general character of the country which gave Linnæus birth, I shall not pause to inquire. Yet in an investigation of this kind, where the opinion of the Swedish Pliny is necessarily mentioned, it might be curious to look to the sources of his credulity. The study of nature, as it respects this globe, is, perhaps, of all the sciences, the most unfavourable to superstition, or credulity. But the greatest of naturalists was one of the most credulous of philosophers.

It is certain that Linnæus was credulous.

It is proper, however, to observe, in this place, that Linnæus by no means asserts, that he himself had ever been a

He never was himself a witness to the fact.

* See Reflections on the Study of Nature, translated from the Latin of Linnæus. p. 33 & 34. Dublin edition, 1786. Dr. I. E. Smith, the ingenious translator of this dissertation, in a note to the above passage, has the following words. "This opinion of the fascinating power of the Toad has been refuted, and the appearance which gave rise to it fully accounted for, by Mr. Pennant, in his British Zoology. Probably the story of the rattle-snake's having a similar power might be found equally false, if enquired into with the same degree of accuracy." p. 34.

† See his *Materia Medica*, liber. i. de Plantis, &c. Amstelædami: 1749.

witness

witness to the fascinating power of any of the serpent-tribe. He seems to have received the tale from some of his many pupils, whom he animated with the love of natural history. It is probable that Kalm, whom Linnæus quotes upon various occasions, and whom he could not but esteem, principally contributed to fix his illustrious master's credulity in this respect: for, in different parts of his *Travels*, this industrious author has given his decided assent to the tale; and he informs us, that he has treated of the same subject, more fully, in a treatise which is printed in the *Memoirs of the Royal Swedish Academy of Sciences*, for the year 1753 *.

Nor was Kalm;
though he sup-
ports it,

Kalm is candid enough to tell us, that he never saw an instance of the fascinating power of the serpent-kind. "However," says he, "I have a list of more than twenty persons, among which are some of the most creditable people, who have all unanimously, though living far distant from each other, asserted the same thing †." He then goes on to tell us a long story, similar to that which I have related, in the beginning of this memoir, and which, therefore, it is not necessary to repeat, in this place.

and speculates
upon it.

Our author is not content to make mere mention of the fact; he undertakes to speculate upon it. And here, although a talent for ingenious reasoning is, certainly, not the most striking feature that is displayed in the *Travels* of Kalm, he acquits himself, for some time, very judiciously; but spoils all he has said, by concluding, that the bird or squirrel "are only *enchanted*, whilst the snake has its eyes fixed on them. ‡" He allows that "this looks odd and unaccountable, though," says he, "many of the worthiest and most reputable people have related it, and though it is so universally believed here," that is in New-Jersey, &c. "that to doubt it would be to expose one's self to general laughter §."

Several American writers have adopted the notion, that snakes are endued with a fascinating faculty. Fearful that their authority may extend the empire of this error, I have been the more anxious to offer my sentiments on the subject to the society ||.

It

* *Travels into North-America*, &c. vol. i. p. 318 & 319.

† *Ibid.* vol. ii. p. 207 & 208.

‡ *Travels into North-America*, &c. vol. ii. p. 210.

§ *Ibid.*

|| Speaking of the rattle-snake, my worthy friend Mr. William Bartram

It has given me pleasure to find, that the enchanting faculty of the rattle-snake is doubted by some very respectable European naturalists. "It is difficult," says my excellent friend Mr. Pennant, "to speak of its fascinating powers: authors * of credit describe the effects. Birds have been seen to drop into its mouth, squirrels descend from their trees, and leverets run into its jaws. Terror and amazement seem to lay hold on these little animals: they make violent efforts to get away, still keeping their eyes fixed on those of the snake; at length, wearied with their movements, and frightened out of all capacity of knowing the course they ought to take, become at length the prey of the expecting devourer, probably in their last convulsive motion †."

Respectable European naturalists doubt it.

My friend Mr. de la Cépède, one of the most eloquent naturalists of the age, has devoted a good deal of attention to the subject, in his *Histoire Naturelle des Serpens*, a work of extensive and superior merit. I regret, however, that this ingenious author was not in possession of a few facts, well known in this country, which could not have failed to conduct a mind, like his, strengthened by the enlarged contemplation of the objects of nature, to the fulness and certainty of truth. As it is, however, Mr. de la Cépède deserves our thanks for reviving, and giving a new turn to, the speculations of naturalists on this subject.

The subject treated by la Cépède.

Bartram says: "They are supposed to have the power of fascination in an eminent degree, so as to enthral their prey. It is generally believed that they charm birds, rabbits, squirrels, and other animals, and by stedfastly looking at them, possess them with infatuation; be the cause what it may, the miserable creatures undoubtedly strive by every possible means to escape, but alas! their endeavours are in vain, they at last lose the power of resistance, and flutter or move slowly, but reluctantly towards the yawning jaws of their devourers, and creep into their mouths, or lay down and suffer themselves to be taken and swallowed." Travels through North and South Carolina, Georgia, East and West Florida, &c. p. 267. Philadelphia: 1791.

* "Lawson—Catesby—Ph. Tr. abridg. ix. 56, &c. vii. 410.—Brickel's Hist. Carolina, 144.—Beverley Virginia, 260.—Colden, i. 12. Dr. Brickel is an author of no credit. His *History of North-Carolina*, here quoted is one of the most daring and scandalous instances of plagiarism I am acquainted with.

† Arctic Zoology, vol. ii. p. 338. London: 1792.

I beg

I beg leave, in this place, to quote that part of Mr. de la Cépède's work which relates to the question of my memoir.

His account.
Supposition of
infection in the
breath.

Speaking of the boiquira, or rattle-snake, my ingenious friend has the following words: "His infectious breath, which sometimes agitates the little animals he is about to seize, may also prevent their escape. The Indians relate, that a rattle-snake is often seen, curled round a tree, darting terrible glances at a squirrel, which after expressing its fear by its cries and its tremour, falls at the foot of the tree, where it is devoured. Mr. Vosmaer (at the Hague), who has made several experiments on the bite of a rattle-snake, which he had alive, says that the birds and mice, which were thrown into the cage, would immediately endeavour to squat in a corner, and that soon after, as if seized with deadly anguish, they would run towards their enemy, who continually shook his rattles: but this effect of a mephitick and fetid breath has been so much exaggerated, and misrepresented, that it becomes miraculous.

Fascinating pro-
perty supposed to
arise from this
cause.

"It has been said," continues our author, "that the rattle-snake had a faculty of enchanting, as it were, the animal he intended to devour; that by the power of his glance, he could oblige the victims to approach by small degrees, and finally to fall into his mouth; that even man could not resist the magic force of his sparkling eyes; and that under violent agitations he would expose himself to the envenomed tooth of the serpent, instead of endeavouring to escape. If the rattle-snake had been more generally known, and if his natural history had engaged more attention, other circumstances, still more extraordinary, would have been added to these miraculous feats; and how many fables would not have been substituted to the simple effect of a pestilential breath, which, however, has by no means been either so frequent or so fatal as some naturalists have believed.

Or that the
animal may have
been already
bitten.

"We may presume, with Kalm, that, for the most part, when a bird, a squirrel, or any other animal, has been seen precipitating itself from the top of a tree into the jaws of a rattle-snake, it had been already bitten*; that after escaping,

* I do not find that Kalm has adopted this system of explanation, in his *Travels*. On the contrary, in this work, he gives some judicious reasons for rejecting this mode of explanation. *Travels*, &c.

manifested, by its cries and its agitation, the violent action of the poison left in its blood, and diffused through its circulation, by the envenomed inoculation of the reptile's tooth; that, its strength gradually decaying, it would fly or leap from branch to branch, till finally exhausted it would fall before the serpent, who with inflamed eyes, and eager looks, would watch attentively every motion, and then dart on his prey, when it retained but a small portion of life. Several observations related by travellers, and particularly a fact mentioned by Kalm, appeared to confirm this*."

From this long quotation, it appears that Mr. de la Cépède adopts two modes, or circumstances, for explaining the miraculous power, which has been attributed to these serpents. The explanation is, undoubtedly, in both cases, ingenious, and entitled to notice. I shall examine the question with that attention which it deserves.

Examination of these doctrines.

In the first place, my learned friend supposes, that the rattle-snake's infectious breath †, by agitating the little animals which it means to devour, may prevent their escape.

I do not altogether understand this expression of an infectious breath. I do not think that we are in possession of any facts by which it can be proved, that the breath of the rattle-snake is, in general, more infectious, or pestiferous, than that of many other animals, whether of the same or of a different family. I know, indeed, that in some of the larger species

Observation to show that snakes do not emit any offensive odour,—

&c. vol. ii. p. 209 & 210. His memoir, in the *Swedish Transactions*, I have not seen. Sir Hans Sloane, a long time since, conjectured, that the whole mystery of the fascinating faculty of the rattle-snake is this, viz. "that when such animals as are the proper prey of these snakes, as small quadrupeds, birds, &c. are surprised by them, they bite them, and the poison allows them time to run a small way; or perhaps a bird to fly up into the next tree, where the snakes watch them, with great earnestness, till they fall down, or are perfectly dead, when having licked them over with their spawl or spittle, they swallow them down." *Philosophical Transactions*, vol. xxxviii. no. 403. M. de la Cépède does not appear to have seen Sloane's paper.

* *Histoire Naturelle des Serpens*, p. 409, 410 & 411. a Paris: 1789.

† His words are, "son haleine empestée, qui trouble quelquefois les petits animaux dont il veut se saisir, peut aussi empêcher qu'ils ne lui échappent." p. 409.

of

of serpents, inhabiting South-America, and other countries, there is evolved in the stomach, during the long and tedious process of digestion in these animals, a vapour, or a gas, whose odour is intensely fetid. I have not, however, found that this is the case with the rattle-snake, and other North-American serpents, that I have examined. But my own observations on this head have not been very minute. I have made inquiry of some persons (whose prejudices against the serpent-tribe are not so powerful as my own), who are not afraid to put the heads and necks of the black-snake, and other serpents that are destitute of venomous fangs, into their mouths, and have been informed, that they never perceived any disagreeable smell to proceed from the breath of these animals. I have been present at the opening of a box which contained a number of living serpents; and although the box had been so close as to admit but a very small quantity of fresh air, although the observation was made in a small warm room, I did not perceive any peculiarly disagreeable effluvium to arise from the bodies of these animals. I am, moreover, informed by a member of this society*, who, has, for a considerable time, had a rattle-snake under his immediate care, that he has not observed that any disagreeable vapour proceeds from this reptile. On the other hand, however, it is asserted by some creditable persons of my acquaintance, that a most offensive odour, similar to that of flesh, in the last stage of putrefaction, is continually emanating from every part of the rattle-snake, and some other species of serpents. This odour extends, under certain circumstances, to a considerable distance from the body of the animal. Mr. William Bartram assures me, that he has observed "horses to be sensible of, and greatly agitated by, it at the distance of forty or fifty yards from the snake. They showed," he says, "their abhorrence, by snorting, winnowing, and starting from the road, endeavouring to throw their riders, in order to make their escape†." This fact related by a man of rigid veracity, is extremely curious; and, in an especial manner, deserves the attention of those writers, who, like Mr. de la Cépède, imagine that this fetid emanation from serpents is capable of affecting birds,

and others to
show that they
do.

* Mr. Charles Wilson Peale.

† MS. note, communicated to me.

at small distances, with a kind of asphyxy*. It even gives some colour of probability to the story related by Metrodorus, and preserved in the *Natural History* of Pliny †.

(To be continued.)

VIII.

*Extract of a Letter from Brugnatelli on the Prussiate of Potash, and on the Preparation of a fulminating Muriate of Silver.**

IF alcohol be poured into a solution of prussiate of potash (oxiprussiate of potash,) the liquor becomes turbid, and a precipitate is deposited in the form of small and very brilliant lamellæ, resembling the sublimed boracic acid (oxiboracic.) Collecting this precipitate on a filter, and drying it, the scales adhere to one another, and form a stratum of a shining yellowish white, like that of mother-of-pearl. This mass easily separates from the filter; is soluble in sulphuric acid (oxisulphuric), and this solution takes a blue colour on the addition of water. Exposed to the fire in a crucible, it swells, burns, and leaves a coally residuum, containing a great deal of iron, attractable by the magnet and potash. This coally residuum, treated with acids, gave out a strong smell of sulphurated hydrogen gas (*gas phlogogène sulfuré.*)

Alcohol occasions a precipitate in solution of prussiate of potash, which is small scales, adhering to each other, of a shining, yellowish white, soluble in sulphuric acid; and this solution is turned blue by water.

Its coally residuum contains a great deal of magnetic iron, and potash; and with acids smells strongly of sulphurated hydrogen gas.

I have applied the process I mentioned to you in my last to a great number of other metallic oxides (thermoxides), in hope of rendering them fulminating; but I succeeded with none, except that of silver.

Take a hundred grains of lunar caustic (fused oxiseptonate of silver) in powder, put them into a beer glass, and pour on them first an ounce of alcohol, and then as much concentrated nitrous acid (*oxiseptoneaux.*) The mixture grows hot, enters into a state of ebullition, and an ether is visibly formed, that changes into a gas. By degrees the liquor becomes milky and opaque, and is filled with small and very white clouds. When all the gray powder of the lunar caustic has taken this form, and the liquor has acquired a consistency, distilled water must

On 100 grains of nitrate of silver powdered was formed first, alcohol 1 ounce, then nitrous acid 1 ounce.

As soon as the precipitate was completely formed, water was poured on, and the precipitate dried in a filter.

* *Histoire Naturelle des Serpens*, p. 355.

† *Lib. xxviii. cap. 14.*

‡ *Van Mons's Journal*, IV. 235.

be added immediately, to suspend the ebullition, and prevent the matter from being redissolved, and becoming a mere solution of silver. The white precipitate is then to be collected on a paper filter, and dried.

Its weight more than 50 grains. It detonates more strongly than fulminating mercury, being touched with a glass tube wetted with sulphuric acid,

placed on a lighted coal, or having a spark drawn through it from the electric pile.

This precipitate is fulminating silver, and amounts to more than half the weight of the lunar caustic employed. The detonating power of this preparation greatly exceeds that of fulminating mercury, prepared in Howard's or my way, even in much smaller quantity. It detonates in a tremendous manner, on being scarcely touched with a tube of glass, the extremity of which has been dipped in concentrated sulphuric acid (*oxisulphurique*), even in that of the shops. One grain of this fulminating silver, placed on a lighted coal, gave such a loud report, that it deafened the ears of the persons present. The same thing happens on placing a little of the same preparation on an electric pile, with the interposition of a bit of paper, and drawing a spark from its centre by means of a slip of metal. The paper will be either pierced or torn.

IX.

Of the Application of Platina or Porcelain. By KLAPROTH,†

Platina never yet applied on porcelain.

PLATINA, as far as I know, has never yet been employed in encaustic painting. Accordingly I thought it not amiss, to make some experiments on the subject, the result of which did not deceive my expectations.

Gold and silver only have been used in this way in the metallic state: and gold answers completely, but silver does not cover the ground so well, and is liable to tarnish.

Gold and silver are the only metals, that have hitherto been employed in the metallic form in painting or ornamenting porcelain, glass, or enamel. Gold answers this purpose so completely, that it leaves nothing to be desired: but silver is far from affording equal satisfaction. As it is of less density than gold, it cannot be used so thin, and does not cover the porcelain or other substance so well. Another reason why it is less applicable to the purpose of painting on porcelain is its property of having its metallic lustre tarnished by sulphurous emanations, all kinds of which blacken it. This unfavourable circumstance prevents silver from being employed in fine enamel painting, and confines the use of the metallic substances in that only to gold.

* Abridged from Scherer's *Journal der Chemie*, 1802, No. 4. p. 413.

Platina in its qualities ranks with gold, and by its whiteness supplies the place of silver, without labouring under its defects. It not only covers the ground well by its density, which even surpasses that of gold; but like this it resists all the alterations of the atmosphere, and is not any way tarnished by sulphurous effluvia.

Platina imitates the whiteness of silver, without having its defects.

The process for applying it is very simple. Platina is dissolved in nitro-muriatic acid, and precipitated by a solution of muriate of ammonia. The red crystalline precipitate formed is to be dried, reduced to a fine powder, and made slightly red hot in a glass retort. The muriate of ammonia, which had precipitated in combination with the platina, sublimes; and the metal remains at the bottom of the retort, in the form of a light, gray powder. This powder being mixed with a small proportion of flux, as is done with gold, and ground with oil of spike, is to be applied to the porcelain, put into the furnace, and afterwards burnished.

Process of applying it.

Platina applied on porcelain in this manner, is of a silver white, slightly tending to the gray of steel. By alloying this metal in different proportions with gold, different shades of this colour are obtained. Platina admits a considerable quantity of gold, before its colour undergoes any perceptible change to yellow. For example, if one part of platina be alloyed with four parts of gold, the presence of the latter cannot be perceived, and the colour scarcely differs from that of pure platina. The colour of the gold does not predominate, unless it be in the proportion of eight to one.

It resembles silver with a slight steel tinge. Gold in different proportions varies its shade; but does not render it perceptibly yellow, unless 4 parts of gold be added to 1 of platina; and the gold colour does not predominate, unless it be in the proportion of 8 to 1.

The alloys of platina with silver give only a dull metal.

Beside this method of applying platina to porcelain, it may be laid on in the state of solution. In this way its colour, lustre, and appearance, are very different. If the nitro-muriatic solution of platina be evaporated to a certain consistence, and laid on the porcelain several times, the metal penetrates into its substance, which, after it comes out of the furnace, exhibits a metallic mirror of the colour and brilliancy of polished steel.

Platina and silver give a dull alloy.

Platina may likewise be applied in solution; and this, if several coats be laid on, penetrates the substance, and produces a mirror resembling polished steel.

Xr.

Abstract of a Memoir on Galvanism, sent to the National Institute by Mr. RITTER, of Jena.†

TO form a just idea of these researches, it is necessary to recal to mind a discovery made by Mr. Erman of Berlin, about two years ago, and since repeated by Volta before the galvanic committee of the Institute.

Erman discovered, that an imperfect conductor would take the electricity from an insulated pile, and become charged like it, one end +, the other —.

If an electric pile, the superior pole of which is positive, and the inferior negative, be insulated, and a communication is made between these two poles, by means of an imperfect conductor, as for instance, a slip of paper wetted with pure water would be for such small quantities of electricity; each moiety of this slip would take the electricity of the pole with which it communicated; the upper part would be positive, the lower negative.

If this be removed by a glass rod, its equilibrium will not be restored at once;

Now let us conceive this imperfect conductor to be removed by an insulating substance, as a glass rod: the equilibrium between the two extremities will not be restored instantaneously, but they will remain positive and negative for some time, as when they communicated with the two poles of the pile.

but gradually.

This difference will gradually diminish in proportion as the opposite electricities recombine, and their action, being neutralized, will soon become altogether imperceptible.

For this Mr. Ritter substitutes a pile of copper and wet pasteboard in alternate pieces.

The fundamental experiment of Mr. Ritter is precisely reducible to this; he merely substitutes for the slip of paper, a pile composed of disks of copper and wet pasteboard. This pile, incapable itself of setting electricity in motion, at least if we suppose all its component parts homogeneous, is charged by communication with the pile, like the slip of wet paper above-mentioned.

Weak electricity, like light, does not pass freely from one surface to another.

Hence Mr. Ritter's pile remains charged for some time;

But there is an essential difference in the results. It appears that electricity, when weak, experiences, like light, a kind of difficulty in passing from one surface to another: at least this seems to follow from the experiments of Mr. Ritter, as he himself observes. The electricity introduced into the pile made with one metal alone, experiences therefore some resistance in passing from the metal to the pasteboard; and this obstacle in-

* Bulletin des Sciences, No, 79, p. 145. October, 1803.

creases, in proportion as the alternations are more numerous. Accordingly, this pile, when once charged, must lose its electricity very slowly, if there be no communication between its poles.

But if a communication between its two poles be established, by means of a good conductor, the current of the two electricities and their combination, taking place swiftly, will produce a discharge, which will act, as in the Leyden phial, by an instantaneous shock. To this effect a new state of equilibrium will succeed, in which the intensities of the different plates will be diminished in proportion to the quantity of electricity that is instantaneously neutralized. These discharges must therefore be reiterated; but growing feebler each time, as the contacts are repeated: but they will soon cease to be perceptible, in consequence of the general equilibrium they tend to restore between all the parts of the apparatus; in a word, the action of this pile depends on its becoming alternately a better and worse conductor, according as the two extremities are made to communicate with each other or not.

As to the mode in which the electricity disposes itself, it must be such, that the repulsive power, or intensity of each plate, combined with the resistance of the surfaces, must counterbalance the united actions of all the rest. In consequence, if the plates of metal be an uneven number, and the whole apparatus insulated, the intensities will diminish uniformly from the two extremities, where they will be equal and contrary, as in the primitive pile, to the centre, where they will be null; but if the base of the pile have a communication with the ground, the intensities will increase throughout the whole of the pile, from the bottom, where they will be null, to the top, where they will equal that of the primitive pile.

The apparatus just described, which Mr. Ritter calls a secondary pile, produces shocks, the decomposition of water, and all the other physiological or chemical effects obtained from the common pile, but with less intensity. By varying the number and order of the disks of pasteboard and copper, Mr. Ritter obtained several interesting results. Thus he observed, that of all the modes, in which a certain number of heterogeneous conductors could be disposed, the arrangement that has least alternation is most favourable to the propagation of electricity. For instance, if a pile be constructed of sixty-four

unless a communication be made between its poles, when it will give a shock, like the Leyden phial.

Thus the intensities of the plates will be diminished; till by repeated discharges, each weaker and weaker, the equilibrium is restored.

Its action explained.

The electricity is greatest at the extremities, nothing in the centre, if insulated; or greatest at the top, and nothing at the bottom, if it communicates with the ground.

This secondary pile produces all the effects of the common pile, but weaker.

The fewer the alternations, the more speedily the electricity flows off,

and the con-
ductivity.

Though weak electricity experiences resistance in passing from one surface to another; electricity strong enough to overcome this passes off at once.

As the arrangement affected the conducting power, it might be supposed it would influence the effects.

And it appeared, that the fewer the alternations, the greater the chemical effect;

and on the contrary, the more numerous the alternations, the greater the physiological effect.

The former therefore is produced by a brisk continued flow of the electricity; the latter by repeated shocks.

plates of copper, and sixty-four pieces of wet pasteboard, disposed in three parcels, all the pasteboards being placed together in the middle, and thirty-two plates of the metal at each end, this pile will conduct the electricity from Volta's very freely, and consequently charge itself very little. If the series of wet conductors be interrupted by a single plate of copper placed in the midst of them, the conducting power will diminish; more frequent interruptions will enfeeble it still more; and thus by increasing the number of interruptions we arrive at a series, in which the conducting power will be scarcely perceptible. These phenomena led Mr. Ritter to the knowledge of the resistance, which a weak electricity experiences in passing from one surface to another: and this resistance takes place only in a weak state, for by a singular property, an electricity strong enough to overcome it opens itself a free passage, and flows off completely.

We have just seen, that the conducting power of an apparatus might be varied at pleasure, by altering its arrangement. It was natural to suppose, that these modifications would influence the chemical and physiological effects of the secondary pile; and Mr. Ritter proposed to himself, to ascertain the differences of this influence.

Accordingly he sought how to divide a given number of wet and solid conductors so as to form a secondary pile, that should receive the greatest possible charge from a given electrical pile. Pursuing this inquiry, he discovered two different arrangements, one of which gave a maximum of chemical effect, the other of physiological. The first consists in a small number of alternations. In this case the conducting power is very great, the flow of the fluid continual, and the physiological effect but weak. The second, on the contrary, consists in more numerous alternations; in which the conducting power is much less, and takes place only at intervals, in momentary discharges, when the resistance of the surfaces is overcome. In this the electricity escapes as it were by jerks, and the chemical effect resulting from it is scarcely perceptible. These differences appear to us to indicate, that the chemical effects depend particularly on a brisk continuous current of the fluid, while the physiological require successive sudden discharges, entering the organs as if by shocks.

From

From these principles we see, why the apparatus of Mr. Ritter is better adapted to separate these two kinds of action than any other. In the common pile the intensity increases with the number of pairs of metal, and balances the resistance arising from these alternations; while in the secondary pile the intensity of the two extremities can never surpass that of the primitive pile, and the resistance produced by the alternations is entirely employed in modifying the current of a given quantity of electricity.

In fine, if Volta's pile can thus change the secondary pile of Ritter, this property is owing to its intensity being extremely weak, and as we may say imperceptible. A more powerful electricity, such for example as that of the common electrical machine, would pass through the apparatus completely and not produce the same effects.

Ritter's pile well adapted to separate the two actions.

But Volta's pile could not charge Ritter's, if its electricity were not very weak.

Though these deductions appear to us very natural, we offer them only with extreme reserve, and because we think they agree very well with the facts observed by Mr. Ritter.

These deductions not advanced as certainties.

The differences that exist in the chemical action of common piles, in proportion to the size of the plates, occur likewise in the secondary piles. The arrangement of the pieces of paste-board, their thickness, the nature of the solution with which they are wetted, and the order in which they are interposed, as well as various other trifling circumstances, modify the effects in a thousand ways, which it would be both useful and curious to investigate.

Causes of difference in the secondary pile.

It would be useful and curious to investigate its effects.

XI.

On Spectacles; particularly the Periscopic. In a Letter from Mr. E. WALKER.

To Mr. NICHOLSON.

Lynn, March 19, 1804.

SIR,

ALTHOUGH the improvements which have been made in optics within the last century, have been such as even to exceed the expectations of a Newton, yet our spectacles, the most useful of all optical instruments, have remained for ages in the same state of imperfection. Many alterations, it is true,

Spectacles have not been improved as much as other optical instruments.

have taken place in their form, but without producing any better optical effect; and this may occasion a suspicion, that the properties of the glasses made use of, have not been clearly understood.

General position
that the rays
ought to fall
perpendicularly
on the lens.

Emerson observes, that "when glasses are put into frames for spectacles, these frames ought not to be straight, so as both glasses may lie in the same plane; but they ought to be so bent in the middle, that the axis of both glasses may be directed to one point, at such a distance as you generally look with spectacles. By this means the rays will fall perpendicularly upon both glasses, and make the object appear distinct. But if they fall obliquely upon the glasses, it will cause a confused appearance in the object *." Martin also bent the frames of his visual glasses, for the same purpose as above described. And it appears from Dr. Wollaston's description of his *Periscopic Spectacles*, that they are also constructed on a principle to avoid oblique rays †.

Affertion:
that the perpen-
dicular rays are
least adapted
to distinct
vision.

It is very singular, that this mode of constructing spectacles should have been recommended by so many writers on optics, when it is easily proved, that the rays of light which fall perpendicularly upon the centre of a lens, enter the eye placed near it, in a more confused state than those rays which fall upon a lens obliquely.

General obs.
of Dr. Smith.

Dr. Smith, in his complete system of optics, gives us this general observation on vision. "That the apparent distinctness and confusion of an object depends upon the mutual inclination of the rays to each other in any one pencil when they fall upon the eye." ‡

Illustration;
by which it is
maintained that
a pencil of rays
passing near the
axis of a lens
will afford vision
less distinct

This observation being admitted, let L L, *Fig. 1.* represent a double convex lens, *ao*, a ray of light passing perpendicularly through its centre; *cn*, and *dn*, two other rays nearly parallel to the former. The ray *ao* suffers no refraction; but the ray *cn* by passing through the lens, will be refracted towards the right hand, and the ray *dn*, by refraction, will be turned towards the left; consequently these rays have not a mutual inclination to each other when they fall upon the eye placed near the lens, as at *x*.

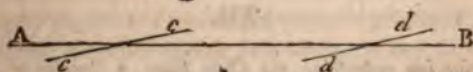
* Emerson's Optics, page 167.

† Philosophical Journal, V. VII. p. 143.

‡ Elementary parts of Smith's Optics, page 76.



Fig. 2.



But, let rs , rt , represent two other rays falling upon the lens in an oblique direction; these rays after passing through the lens, will both be refracted towards the left hand, and will therefore have "a mutual inclination" to each other when they fall upon the eye at y , turned to view an object in the direction ry ; hence an object viewed in that direction, will be seen more distinctly, than if it were viewed through the centre of the lens.

The same thing may be proved experimentally, thus; let a person when reading with spectacles that are proper for his sight, look attentively for a moment, at a single word through the centres of his glasses. Then let him turn his head, either to the right or to the left, (without moving the book) so that the same word may be seen by oblique rays, and it will appear much more distinctly than before.

From what has been advanced it appears, that when glasses are put into frames for spectacles, those glasses should stand in an oblique direction, that the rays of light issuing from an object, be fixed obliquely,

—than a pencil passing farther from the center.

Experiment offered in support of this doctrine.

Inference: That spectacle glasses ought to be fixed obliquely,

ject, may not fall perpendicularly upon them. Bending the frames will not effect this purpose, for the glasses must be parallel, as represented in *Fig. 2.* where *AB* represents the frame, *cc* and *dd*, the two glasses.

I have a pair of spectacles constructed in this manner, but so contrived, that the glasses may be set to any angle, to show the different effects of oblique and perpendicular rays.

The theory of Dr. Wollaston's patent spectacles, may be thus examined.

and that the meniscus is not superior to the double convex.

Let the lens *LL* be taken away from *Fig. 1.* and a meniscus of the same diameter and focal distance be drawn in its place. Then the rays *rs*, *rt*, after passing through this lens will meet at (*p*) as before, because this lens is of the same focal distance as the double convex, *by Hyp.*

Consequently these rays will fall upon the eye at *y*, in the same state of convergency as before. Whence a lens of the meniscus form, possesses no properties, *in theory*, superior to the double convex,

E. WALKER.

XII.

*Communication respecting the Chay Root, a Species of Madder used for dying durable red Colours in the East Indies. By JOHN STOCKWELL, Esq.**

Introductory letter to Mr. Taylor.

MR. STOCKWELL presents his compliments to Mr. Taylor, with a sample of an article called Chaya Root, for the inspection of the Society for the Encouragement of Arts, &c. It is in much use on the Coromandel coast, for dying. He accompanies it with a short extract, which will afford some information concerning it.

If Mr. Taylor should cause any experiments to be made upon it, Mr. Stockwell will be obliged by being favoured with the result; and if any person should think favourably of it, and be desirous of a quantity, no doubt it may be had.

East-India Warehouse,
Crutched Friars, 23d May, 1803.

* Made to the Society of Arts, Adelphi, and printed in Volume XXI. of their Transactions.

Extract from the Bengal Commercial Consultations, the 24th of April, 1798.

To W. A. EDMONSTONE, Esq. Secretary to the
Board of Trade.

SIR,

I HAVE the honour to acknowledge the receipt of your letter of the 31st ult. informing me that I was desired by the Board to make some experiments with the Chay Root, and to report whether I think it will be of any use to the dyers or calico-printers at home.

Letter to the
Bengal Board of
Trade.

It is rather surprising, considering the pains that have been taken in Europe to discover, or at least to imitate the method of dying the *Turkish* or *Adrianople* red, that so little attention has been paid to the equally beautiful and permanent red given to their calicoes by the natives of the coast of Coromandel. Although full accounts of the practice of calico-printing in the East Indies were sent home long ago by the missionary Cœur Doux, M. Poivre, and others, it does not appear that the European artists have ever tried their skill in the *Chay* root, the drug by which the admired red colour is produced. I have never heard, at least, of any such attempts, nor do I believe that the root has ever been sent home. It is evident, from the manner in which this drug is mentioned by Dr. Bancroft, in his "Experimental Researches," p. 174, that well-informed writer had never seen it, which I think could scarcely have happened, had it been at all known to the London dyers.

The Chay-root
affords a beautiful and permanent red to cottons.

Not practically
known in Europe.

It is probable that the tediousness of the Indian process, as described by those who sent home the accounts of it, consisting of many tiresome manipulations, continued during a period of nineteen or twenty days, deterred the European artists from trying the effects of the *Chay* root in dying or printing their cottons. What may appear more extraordinary is, that the same cause, co-operating perhaps with the natural indolence of the people, and their having cheaper though inferior red dyes at hand, has prevented the use of this root from obtaining in Bengal; for, so far as I can learn, it is not used in this part of India. The natives here are content with the red produced by the *Aul Munjiet* and other drugs, though the colours yielded by these are far inferior to those of Madras calicoes.

The Indian process apparently tedious.

Cheaper drugs
used at Bengal.

On

On this account I am surpris'd at the measure which has been adopted, of sending round hither such a large quantity of the *Chay root* from the coast. I apprehend very little or none of it will be purchased at the approaching sale, in which case it must be either returned to Madras or sent to England. The expediency of this last measure must depend on the possibility of so abridging the process of dying, or printing cottons with *Chay root*, that the English artists may find it worth their while to have recourse to it for the reds and purples, instead of the *Smyna madder*, or whatever other drugs they use for those purposes at present.

Experiments to
shew whether
the *Chay root*
might be of va-
lue in Europe.

To ascertain this point, I have, agreeably to the instructions of the Board, made several experiments with the *Chay root*; indeed as many as the shortness of the time, and my other avocations would permit. From the result of these trials, I entertain great hopes that the English calico-printers will not only shorten the process, so much as to finish it within a tenth part of the time required for the Indian process, but that they will by the *Chay root* dye their cottons of a brighter red than can be done by madder, or any other vegetable.

Cotton was
burred or galled;

It is needless to detail the many trials I made, which either failed altogether, or succeeded in a very imperfect degree. I shall therefore only mention that process which I found to answer best, and by which the piece of cotton cloth, which I herewith send you for the inspection of the Board, was printed. Having made a decoction of two ounces of powdered *Hurr*, (the fruit of the *Myrobolona Citrona**) in a quart of water, I took a piece of Madras cotton cloth, and boiled it in the decoction for about half an hour. Having taken out the cloth, and washed it well with cold water, I dried it in the sun, and afterwards had it properly ironed and smoothed for the pencil.

then washed;

and a design
made with ace-
tite of alumine;

washed,

I then took some of the acetite of alumine, made in the manner directed by Dr. Bancroft, and, thickening it properly with gum-arabic, I delineated a flower with this mordant upon the cloth, and dried it in the sun. I afterwards washed the cloth in cold water, to clear it of the superfluous acetite, and dried it again in the sun. I then infused about two ounces of the *Chay root*, coarsely powdered, with about a quart of water, in a vessel well tinned, and setting it on the fire, as soon as the

* Aleppo galls will probably answer as well as the *Hurr*.

liquor began to grow warm, I put the cloth into it, and let it remain until it had boiled about half an hour, during which the cloth was frequently stirred. I then took it out, and having rinsed it well with cold water, I put it to dry in the sun. The delineation of the flower now appeared of a good bright red, and the ground of the cloth, though slightly tinged, was much less so than I expected. By washing it again with cold water, and afterwards with soap and water, and exposing it for a whole day to the sun, during which it was frequently besprinkled with water, I brought it to the state in which you now see it. The whole process has only taken up about six and thirty hours. I should have mentioned that the washing with soap heightened the brightness of the red considerably.

and boiled in the infusion of Chay-root.

It gave a good red;

which stood washing, was improved by soap, and cleared by exposure to the sun.

Making allowance for my want of experience in the practice of dying, and considering the great improvement which may be expected in the process from the superior skill of the English artists, I think we may conclude, even from this imperfect essay, that the *Chay root* will be a valuable acquisition to the English calico-printers. I therefore recommend, that so much of the root as may remain unpurchased at the sale, or at least that a part of it may be properly packed up and sent to England by one of the ships now under dispatch.

This rough essay is encouraging.

This drug is the root of a plant called by the botanists *Oldenlandia Umbellata*. I have not met with it in this part of India; but it grows naturally on the coast of Coromandel, where it is also cultivated in great abundance, for the use of the dyers and calico-printers.

Some account of the drug.

The sample which you sent me appears to be of a good quality, and in good condition. It is said that the root will remain with its virtues entire for several years, and that they are even improved by keeping. If this be the case, and provided the dyers at home find it answer, this circumstance is a very favourable one, and must enhance the value of the drug as an article of commerce.

I am, &c.

(Signed)

J. FLEMING,
Inspector of Drugs.

Export Warehouse,
6th April, 1798.

Minute

Minute of the Board of Trade, dated April 10, 1798.

Opinion of the Board of Trade, that the root will be acceptable to calico-printers, and of value to the E. I. Company.

THE Board are of opinion, that the result of the Inspector's experiments will afford very acceptable information to the calico-printers in England; and, if their operations should prove his ideas to be well founded, the plant may prove a valuable acquisition to the manufacturers of Great Britain, and also an article of commerce from the coast particularly useful, as there is a want of coast articles of low value, as well light, as a sufficiency of ponderous, to make up a proper cargo for a large ship, without swelling its value to too great a risk, as would be the case were a ship to be loaded entirely with piece goods.

XIII.

Description of an Apparatus for raising Water by the Fall of Waste Water. By Mr. SHARPLES, Portrait Painter, Bath. Communicated by the Author.

Advantages to be derived from the fall of water.

Small mills in Portugal.

A very small brook may do work,

EVERY drop of water that falls may be considered as a natural power capable of doing some work; and it would be productive of considerable benefit in many domestic and manufacturing exigencies, if small streams and currents descending along the sides of hills, or even through the meadows, were employed, by the application of simple machinery, to assist the labours of man. I am informed that numerous little mills for grinding corn are distributed over the face of the country, in Portugal, upon very inconsiderable rivulets; where they not only add to the general product of human work, but may also perhaps tend to equalize the price of grain to the consumer. And indeed if it be considered that a small stream of about a yard wide, running over an obstacle to the depth of an inch, and having space to descend through twelve inches, would do half as much work in the 24 hours as a horse, whether in grinding corn or turning an engine, or raising water for useful purposes, we shall be struck with the necessity of enquiring whether any falling water whatever should be suffered to descend without due employment in its course.

The

The ingenious gentleman who has favoured me with the sketch from which Plate XVI. was made, has directed his invention to teach us how the waste water of our apartments, and other similar places, may be made to raise other water in its descent; and the simple engine he proposes for that purpose consists of the following parts:

A, B, and C represent three vessels or compartments. The upper compartment A is an open trough, or waste receptacle, which can be emptied into the second close vessel B, by turning the cock D; which cock, by the same motion, opens the communication-pipe I and an air-pipe H, to suffer the included air to issue out. N and M are the two legs of a syphon; the shorter N, communicating with the reservoir O, containing water, and the longer M, communicating with a trough P, at a lower level which overflows into the drain. The cock F, by the same turn, opens or shuts both these pipes at once. And lastly, the pipe G forms a communication from the upper part of the receptacle B to the upper part of C. The process may be thus explained:

When D is opened, the middle vessel B receives waste water from A. If D be then shut and F opened, the longer syphon-leg M admits the waste water to flow into P; leaving a space in B wherein the air in C as well as B expands (with a diminished spring) by means of the pipe G. The pressure of the atmosphere on the water in O will therefore raise that fluid into C, provided the height be not too great; and if the difference between the heights of the vessels B and C be equal to that between O and P, the quantity of fresh water raised may equal that of the waste which descends from the greater height.

The cock E serves to draw off the water so raised; at which time it is convenient that the cock F should be shut; and this should also be done whenever the cock D is opened. This may easily be made to take place by a simple connection between the cocks, without depending on the skill or diligence of the servant.

W. N.

Engine for employing waste water.

Description.
It is a syphon with compartments.

The foul water runs down one leg, and clean water ascends by the other.

XIV.

*On Gridiron Pendulum Rods composed of Lead and Iron. By Mr. BENZENBERG *.*

MR. B. was induced to employ lead on account of its great dilatability, which is to iron as 2.57 to 1, so that 16.5 inches of lead compensate 1.3 of iron; and he chose iron in preference to steel, because easier to work. The compensation was made by a single rod in the centre, $16\frac{1}{2}$ inches long, French measure, and $\frac{7}{8}$ an inch thick. It was simply pinned into gorges in the cross-piece of copper; but the other parts of the gridiron were rivetted in the usual way. The iron rods were made of the best thick iron wire.

Its advantages. The materials of this pendulum are cheap, and it may be made in a couple of days. As the pressure takes place in a vertical direction, there is no danger, according to Mr. B. of rods of these dimensions bending.

Mode of correcting the compensation. To correct the compensation, the central rod of lead must be left so long that we may be sure the compensation is in excess. The quantity of error may then be found by the freezing apparatus, and how much it is requisite to cut from the rod may be calculated with the greatest exactness.

XV.

On the Advantages of a large Knife in making Pens. By a Correspondent.

To Mr. NICHOLSON.

SIR,

USEFUL discoveries are sometimes derived from the common occurrences of life; and that which I am now going to mention, is a mere accident of this kind. About three years ago, I took up a large pocket knife which had been newly ground, and used it for making a pen, and was not a little surprized to find that it answered the purpose much better than

The edge of a large knife is better than that of a small one.

* *Voigt's Magazin fuer den neuesten Zustand der Naturkunde*, Vol. IV. p. 787.

my penknife I ever had. I mentioned this circumstance soon after to the cutler who made the knife. He said that he had no doubt of the truth of my observation, as he could harden and temper a large blade better than a small one; because there is more difficulty in ascertaining the proper degree of heat in the latter than in the former.

And it may perhaps be a mechanical truth, that a strong knife will overcome the resistance of a hard quill with less exertion of the hand than a weak one. The blade of the knife that I use, and which gave rise to these observations, is four inches in length and half an inch in breadth.

It may perhaps be supposed that a knife of these dimensions is improperly constructed for cutting the sides of a pen into a curve, but it will be found, on trial, that the breadth of the blade is no impediment, for it is the form of its edge which it receives by whetting, that renders a broad blade quite as manageable as a narrow one.

And perhaps the mechanical effect.

It may with convenience and advantage be used as a penknife.

I am, SIR,

A constant Reader of your

Truly valuable Journal,

March 22, 1804.

W.

XVI.

Communication respecting an Article sent from the East Indies, under the Name of Gum Kuteera, and of which a large Quantity has been lately imported into this Kingdom. By Mr. JOHN COWIE.*

To Mr. TAYLOR.

SIR,

THERE is a gum produced in several parts of Oude and the adjacent provinces, so nearly resembling gum tragacanth, as to have been taken for it by many; and large quantities of it, of late years, have been imported into Europe, under this mistaken opinion. But it is now well ascertained, that this gum (which in the country language is called kuteera) is the produce of a particular tree, of quite a different species from the

Gum resembling gum tragacanth,

* Society of Arts, Vol. XXI.

but not so glutinous, or applicable to the same uses,

thorny bush which yields the tragacanth; and being imperfectly soluble, and possessing but little of a glutinous nature, renders it inapplicable to the purposes for which gum tragacanth is used. I am nevertheless of opinion, that this gum might be found serviceable, in one way or other, to some of the arts and manufactures, and am therefore induced shortly to describe it, and the use to which it is applied in India, with a view of exciting any of your ingenious correspondents to make experiments on it, and determine its real value, or absolute inutility.

Description of gum kuteera.

The kuteera is in loose wrinkled drops or pieces, void of smell and taste, of a whitish colour, and mostly transparent. In water, it slowly forms itself to a pulp or jelly, and is nearly tasteless; but, if pounded well in a mortar, and then boiled in water for fifteen minutes, stirring it all the time, it will be found *completely dissolved*. A tea-spoonful of this powder gives to three pints of water the consistence of capillaire. Might it not in this state be of service to painters and artificial florists, or even used to give a gloss to silks? The natives of India make a varnish by mixing kuteera with other gums; and I have been told, they likewise make use of it in the printing of calicoes. It is the principal ingredient in a medicine they give to their horses in certain diseases, and in this respect is of very general consumption.

Large quantities are now in the India warehouses.

I shall only add, that many tons of this gum are now lying in the East India Company's warehouses, totally unsaleable, or that will not sell for more than the first cost in India; and if this notice shall be attended with beneficial consequences, I shall derive great satisfaction in the reflection of having served the interest of the speculators in this drug, amongst whom I could formerly number myself; and I believe I was the first importer of it into this country.

I am, SIR,

Your most obedient Servant,

Finchbury-square,
25th May, 1802.

JOHN COWIE.

XVII.

Experiments with Ice, and a Method of rendering the electric Attraction of the Pile very evident. By S. P. LIER, of Jodoigne, Member of the Society of Emulation, Natural History and Physics, at Brussels.*

I LED myself of the first frosts this year to make some experiments on ice, both as an interposed substance, and as an interposer and conductor.

Experiment 1. A pile was formed of eighty pairs of plates of silver, and as many very thin disks of ice. It not only did not give the slightest shock, but did not excite the electric fluid on the tongue, or produce the smallest luminous effect. I left it standing for several hours, but found no difference in the result.

2. I arranged ninety disks of ice in pairs with as many pieces of silver, and placed between the pairs pieces of pasteboard impregnated with a solution of salt. This combination produced no more effect than the preceding.

3. The same number of disks of ice were paired with disks of zinc, and wet pasteboards interposed as before, with the same result.

4. I made a pile of a hundred and twenty-eight pairs of zinc and silver, interposing pasteboard impregnated with a solution of salt. This pile gave such strong shocks, that they were felt to the shoulders with violence. On forming the circle with the hands armed with little pieces of ice, all attempts at giving a shock were intercepted. I then took a piece of ice in my mouth, and brought it into contact with the top of the pile, while with my hand I touched its base; no electric taste was perceived on my tongue.

5. I have now prevented my pursuing this inquiry any farther. On my next return, I will pair disks of silver with cast disks of zinc, of lime, of pure potash, and of sulphur of potash, to produce in the first two cases an action of physical nature, and in the last a chemical action, after the manner of the experiments of Volta.

Journal de Chimie, par Van Mons, for January 1803, No. XI.

Exp.

On a pile of 140 series, a delicate magnetic needle was suspended; on approaching the hand to it, an oscillatory motion took place, but the magnetic attraction appeared to be stronger than the electric.

Exp. 5. On a pile of 140 series of zinc, silver, and paste-board, impregnated with a solution of salt, I placed an iron pivot, and on this pivot a very delicate and sensible compass needle. The friction was almost nothing. I first applied one hand to the base of the pile, and I carried the other near to the needle. I observed a slow and oscillating approach; but the needle was incessantly invited to resume the magnetic direction, whence I inferred, that the polar attraction exceeded the electric. I tried then to render the transmission of the fluid more easy, by making it glide over a metallic conductor, as a brass wire; but the result was the same as when I made the fluid pass through my arm.

A common pin, suspended in the same way, was attracted by a wire communicating with the pile, at the distance of some lines.

Exp. 6. To the magnetic needle used in the preceding experiment, I substituted one made of a pin, the movement of which also I rendered very free. I approached this needle with the extremity of a brass wire that communicated with the base of the pile, and I perceived it turn with rapidity, at the distance of a few lines, to rush against the wire. A similar communication established through the arms afforded the same result.

A silver thread, suspended from a knitting-needle, placed on the summit of the pile, was attracted by the finger.

Exp. 7. I placed on the summit of the pile an iron knitting-needle, bent, and suspended to it a silver thread, as it is called, used for embroidery. I completed the circle with one of my fingers, approaching the thread gradually with it. At a certain distance the thread applied itself to my finger, and remained adhering to it, though it was perfectly dry.

When the communication was made with a brass wire, vivid sparks were given out, the silver was oxidized, and part of it melted. The same effects took place repeatedly.

Exp. 8. I repeated the preceding experiment, only forming the communication by means of a brass wire. The attractive effect equally took place, and very vivid sparks were given out between the wire and the thread, which oxidized the latter over all its surface, and melted a part of it several lines in length.

All these experiments were repeated a great number of times, and always with the same success.

A silver thread, suspended from a wire fixed to the summit of the pile, was attracted by one hand, when the other was dipped

Exp. 9. In the upper disk of a pile of 97 pairs of zinc and silver, I fixed an iron wire, and suspended to it a similar silver thread. At the foot of the pile was a communicating basin, in which was a solution of salt. Dipping one hand into the basin, and bringing the other near the thread, this immediately came and stuck to it. When I took my hand out of the basin,

the

the thread immediately quitted the other, but stuck to it again when I redipped my hand into the basin; and so on alternately.

Exp. 10. I repeated the preceding experiment, fixing a brass wire, in order to obtain more firmness in the contact with the thread, into a block of lead, placed on the table on one side of the pile. I bent the wire into a crook, and suspended from it an embroidering thread, which I placed at a small distance from the knob of the pile. As often as I held one of my hands wet against the block of lead, and dipped the other into the water in the basin, the thread applied itself to the button of the pile, and stuck there; but it quitted it as soon as I interrupted the communication by withdrawing either hand. This experiment, repeated more than fifty times following, was constantly attended with the same result.

A basin of salt and water communicating with the pile.

A silver thread, suspended from a wire fixed in a block of lead on the table, was attracted by the knob of the pile, when one hand was dipped into the basin, and the other, wetted, touched the lead; but on removing either hand, the effect ceased.

XVIII.

Experiments on the Activity of a Galvanic Pile, in which thin Strata of Air are substituted instead of the Wet Bodies. By Mr. DUCKHOFF*.

MR. RITTER has observed †, that all substances which, when interposed between the constituent parts of the pile, excite electricity, are active only so far as they contain moisture; and he asserts, that it is impossible to construct an active pile without the intervention of a wet substance. I take the liberty to oppose to this assertion an experiment, by which it is contradicted. I constructed a pile with disks of copper and zinc, and little bits of thin green glass, about the size of a lentil, three of which I placed triangularly in the intervals that separated the metallic plates. Thus between each pair of metals I had a thin stratum of air instead of a wet substance; and the following were the results:

Ritter asserts, that moisture is necessary in Volta's pile.

But a pile of copper and zinc in pairs, separated by bits of glass, so as to interpose air alone, exhibits electricity;

1. A pile of ten pairs, tried by the condenser, affected the electrometer as powerfully as a common pile of five pairs.

though only in a degree equal to that of a common pile half as high.

* *Voigt's Magazin fuer den neuesten Zustand der Naturkunde*, Vol. IV. p. 791.

† *Intelligenzblatt der allgemeinen Litteraturzeitung*, 1802, No. 193.

Damp air did not answer so well as dry, apparently.

The electricity was transferrable to the Leyden phial.

The weakness of the electricity perhaps owing to the roughness of the plates.

No reason why glass, &c. should not act in the pile, as in the condenser.

The glass must be as thin and even as possible, that the stratum of air may be so.

2. The action remained invariable as long as the air continued dry; but damp air appeared less favourable to the pile.

3. The same degree of electricity was communicated by the pile to a Leyden phial.

The plates were only three inches in diameter. As they had already been used for a pile in which wet substances were interposed, and were much corroded, I took off the oxidized portions by a file; but the plates still remained rough and uneven; and to this circumstance I ascribe my not obtaining a higher degree of electricity. I have yet had no opportunity of repeating the experiment on a larger scale; but the result I obtained appeared to me sufficient to render the assertion of Ritter at least questionable. I see no reason why glass, thin strata of sealing wax, or the like, should not have the same effect on Volta's pile as in the condenser. For the experiment to succeed, it is necessary that the bits of glass should be as thin and even as possible; for, if the stratum of air be too thick, it is natural that the electricity, which is excited in such a feeble degree by the contact of the two metals, should remain inactive on the adjacent metallic pairs.

XIX.

Extract from a Letter from Mr. J. B. VAN DEN SANDE, Professor at Luxembourg, on the Efflorescence and Decomposition of Glass Tubes.*

Oxidation of white glass not uncommon.

Old barometer-tubes exposed to flame, turned white and opaque;

and the same if simply heated.

I HAVE lately had an opportunity of observing at leisure the common phenomenon of the calcination of white glass. I exposed to the flame of an enameller's lamp some old barometer tubes that appeared not to have any defect, in order to bend them into syphons. Scarcely had they come into contact with the flame, when they turned white and opaque. One of the tubes being wetted, I placed it on a chamber-stove to dry; and before it was heated, it was covered with a whitish efflorescence. Heating the others produced on them the same effect. I then removed the efflorescence, replaced the tubes on the stove, and they became white afresh. By continuing the ex-

* Journal de Chimie, par Van Mons, IV. p. 232.

periments, I reduced an eighth part of the glass to a very fine $\frac{1}{8}$ of the glass powder; after which the tubes were opake, and had lost their polish. thus effervesced,

The greater part of this efflorescence exhibited in the microscope rhomboidal parallelograms; the rest triangles, most of which were isosceles. in rhomboidal parallelograms or triangles.

Having poured nitric acid on a portion of the effloresced matter, it dissolved 0.20. One part of this solution afforded, on evaporation, crystals of nitrate of soda: the other afforded no precipitate with potash. The efflorescence was soda.

These tubes were become good conductors of the electric fluid; which led me to presume, that their decomposition was occasioned by the humidity of the place where they were kept; which humidity, after it had penetrated the surface of the glass, imperceptibly insinuated itself into its pores. This efflorescence, however, depends much on the quality of the glass; for we may observe in the same glass frames, with a south or south-west exposure, some panes calcined to such a degree as to become entirely opake, while others retain their transparency, and are perfectly intact. These tubes had become good conductors of electricity; the decomposition therefore owing probably to humidity. But the efflorescence depends much on the quality of the glass.

XX.

On the Criteria or due Discriminations of Cyder Fruit. By
THOMAS SKIP DYOT BUCKNALL, Esq.*

TO CHARLES TAYLOR, Esq.

SIR,

HEREWITH you receive a paper on the criterions or due discriminations, by which the valuable cyder fruit may be ascertained, referring to my last and preceding Papers on Varieties. On cyder fruit and the making of cyder.

Report says, cyders are not well produced out of the cyder countries. Were it said they are not well made, the assertion would be just; but it is a mistake to suppose that cyder cannot be made in perfection out of those situations. Only choose the proper soil for the fruit, see it ground down to a perfect smoothness, rind, pulp, kernels and all; duly fermented.—Tun the liquor at the exact point of time with regard to settling, which

* Soc. Arts, Vol. XXI.

On cyder fruit
and the making
of cyder.

is well understood in the West, and not attended to elsewhere; and do not rack it too much. Keep the liquor in quantity to a good age; and I take the liberty of maintaining, that there are places in Hertfordshire capable of producing as good and fine cyder as any in Herefordshire.

I was once asked what are the impediments to a more general fine crop of fruit? My reply was, the coldness and uncertainty of the climate, injudicious planting, with inattention and neglect in those who have the management of it. As for planting and guarding against cold, enough has been said on that subject through the whole of my treatises. It has been observed the cyders were better one hundred years ago than they are now. That may have been the case; and I should account for the difference as follows:—When the gentry and superior yeomanry of the country depended on barley and fruits to make an agreeable beverage for their friends, it is natural to suppose that they personally superintended the business themselves, and received great satisfaction when their labours were attended with success; whereas, when Port wines became the luxury of the country, cyder, of course, naturally fell into disuse at the better tables. Hence the masters of families were no longer zealous to produce fine potent liquor, and giving it as cyder, that being assigned to inferior uses.

Undoubtedly, where cyder is the general drink of the country, great quantities must be raised for that purpose; and it is done at so low and easy a rate, as to be sent from the mill to the neighbours' cellars under three half-pence the gallon. What I am attempting is, to be able to produce, *at a certainty*, a fine generous liquor, such as may do credit to the name of the cyderist, and which will depend much more upon attention than is generally imagined.

After having expatiated on the old and new varieties of the valuable cyder fruits, in the last volume of the Society's Transactions, I have nothing farther to offer for the purpose of closing the Orchardist, than to present my best respects to the Society, and give them the marked criterions as follow:—

My friend, Thomas Andrew Knight, Esq. has made good discriminations in his Tract on the Apple and Pear. The Somerset report has further extended the subject.

I say, choose an apple naturally small, of a whitish colour, somewhat tinged with red; of a fine yellow pulp, with a cer-

tain degree of astringency, and as much saccharine matter as nature is disposed to produce; then observe if the cells are large, and full of ripe kernels; and further still, know whether the blossoms are patient of cold, and the fruit ripens well.

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and the making
of cyder.

Such an apple, particularly if a new variety, properly ground down and duly fermented, must make good cyder; and both as to profit and use, be valuable in any neighbourhood, or as an article of trade. The foundation is now so well laid, in the seedling beds in the county of Hereford, that within five years there will be more than one hundred new valuable varieties produced. I last autumn saw two most beautiful new apples, of the first year's growth, upon the Grange estate, and which decidedly obtained the Hereford premium; they were seedlings of high promise. There is an emulation among gentlemen of that part of the country, which does them great credit.

Dr. Symonds gives several of these discriminations; and in addition says:—The flavour of a good cyder-fruit cannot be mistaken by a man conversant with apples, though difficult to be described; but above all, he recommends to choose apples, the rinds of which have follicles, or cells, containing large quantities of essential oil, more particularly to be noticed in the old fire, golden pippin, and paufon, and from which Dr. Symonds conceives the cyder in a great measure derives its flavour. It is to be observed, that the old paufon, woodcock, and red musk, are generally large apples. The old scorched-harvey is an exception as to the yellow colour; that being white, with a brown skin. There are other exceptions; but the material criterions are here enumerated according to the received opinion.

Had I formed these distinctions when I was a boy, I should then have said, take any apple bordering on the golden rennet or golden ruffet, and it will make good cyder, in consequence of supposing that the golden colour is of service. We had a fine apple, the royal pearmain; it was more flat and large than the Seville orange; it had a thin skin and quick taste, with much sweetness, and made fine cyder: but I presume that variety is now over, as I have not seen one of them for many years.

In the October of 1801, which I spent in Kent I was shown a new apple. The tree is handsome, and the fruit has most of the

On cyder fruit
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the criterions, but does not ripen well, which is a great defect in cyder making; we named it *The Bland of Hartlip*. It has the macula, or follicles in the skin, more visible than on the golden pippin. I intend examining it with attention at a future period, as it is not possible to form a certain judgment on it for some years. My reason for mentioning the Bland of Hartlip is, to prove that the idea of searching after new varieties has made a general impression throughout the kingdom.

It was the custom formerly, if a new apple had many good points, but ripened late, to preserve the tree as a new variety, imagining that, as it advanced in age and acquired strength, the fruit would ripen the sooner, and consequently the cyder be richer. I should add to it, lay the land dry, and spread plenty of manure. These attentions would much accelerate the wished-for object of improving the liquor, as well as increasing the quantity of fruit.

I remain,

DEAR SIR,

Your obedient Servant,

THO. SKIP DYOT BUCKNALL.

Hampton-Court, Nov. 14, 1802.

XXI.

*Concerning the Steam Engine from Mr. Blakey, by a
Correspondent. B.*

TO MR. NICHOLSON,

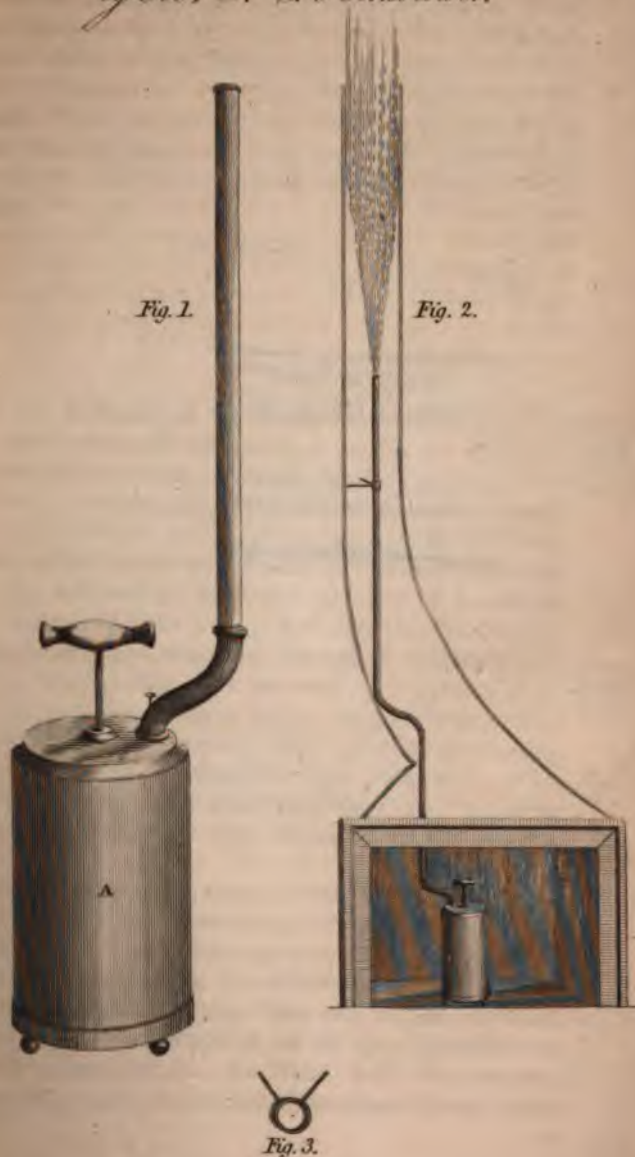
London, March 20, 1804.

SIR,

Introductory
letter.

SOME time about the year 1776, I met with an engineer of the name of Blakey, in Holland, of whom Ferguson the philosopher and several other ingenious men have spoken in high terms. I do not know whether there is any history of him or of his works; but as many of his contemporaries must still be living, I should hope that this notice may produce some information through the medium of your Journal. I have lately met with a pamphlet by this author, intitled "*A short historical Account of the Invention, Theory, and Practice of Fire Machinery;*" printed at London, in the year 1793, without book-seller's

*Apparatus for Cleansing Chimnies,
by M^r. J. C. Hornblower.*



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seller's name. It chiefly consists of short notices of the author's own operations, with academical testimonies; and I understand from private information, that it was published at that time by Mr. B. in indigent circumstances, with a view to obtain subscriptions for a large treatise, which has not since appeared. Some of your correspondents may probably favour us with an account of the disposal of those writings since his decease. I have extracted the outline of history of the steam-engine from the beginning of his pamphlet, which, if you think will be acceptable to your readers, is very much at your service.

I remain, Sir,

Your obliged Reader,

R. B.

IN the beginning of the seventeenth century, a modern philosopher conceived the notion of making use of fire and water to obtain motion. This was Brancas of Rome, who contrived a stamping-mill to be worked by steam, coming from a large æolipile, as may be seen in his twenty-fifth plate. We are obliged to him for a number of ingenious inventions, which he dedicated to M. Canci, governor of Loretto, in 1628, and published his work at Rome the year following.

Brancas first
applied Steam to
mechanism; or
proposed it, 1627.

Thirty-four years after, the Marquis of Worcester published a little treatise, under the name of "Century of Inventions, printed in London, 1663;" he gave them as if he had tried them, and proposed other engines of great utility, which only wanted encouragement from government: "Several were nothing but projects," says Desaguliers, "but, where he is more explicit, there is one for raising water by means of fire."

Marquis of
Worcester,
1663.

In the acts of Leipzig, 1690, p. 410. is to be seen an invention, intitled, "A new manner to procure a considerable quantity of power with little expence, by Dr. Papin." "In this new engine," he says, "of which we have given a description in the acts of Leipzig, 1688, and the intent of which was to obtain a new method of making use of gunpowder as a power to give motion, the chief fault was, that the gun-powder, in the under part of the tube, did not entirely fill

Papin, 1690,

fill

proposed a piston
and condensa-
tion.

fill the capacity of this tube, so as to have no air under the piston *." Papin continues to give reasons like a man of great understanding; but as there must be plates and descriptions to explain the whole, I shall say no more on the subject, but only add, that the doctor did his best to obtain his end in another manner. He shews that, with "A little water changed into steam by means of fire, we can have an elastic power like air; but that it totally disappears when chilled, and changes into water; by which means he perceived, that he could contrive a machine in such a manner, that, with a little fire, he would be able, at a small expence, to have a perfect vacuum, which could not be obtained with gunpowder." Among various contrivances, this appeared to answer the best. In the work itself a plate of this contrivance will be given, which, however, is not to be compared to what has been invented since Papin, by men who had never heard of him, and were altogether much inferior to him in theoretical and practical philosophy.

Capt. Savery,
1699.

In 1699, a remarkable year for the honour of English ingenuity, Captain Savery shewed to the Royal Society an engine which raised water by means of the expansion and condensation of steam; he published a little treatise, which he named "The Miners' Friend;" the description of this engine is to be seen in Harris's Dictionary of Arts, &c. printed in London, 1704. This learned man says of Savery's engine, "that with little art it can be kept in action as long as one will, and that it cannot be hurt but by stupidity and negligence." Harris looked on this invention of Savery's to be one of the most ingenious combinations that had ever been made in hydraulics, and I may add, that it is surprising that a seafaring gentleman, in all likelihood little used to the working of metals, should have brought his invention to the perfection he left it in.

* It is seen that Papin is the first that thought of a piston to be applied in fire machinery, though it was but in a tube about two inches diameter, yet it shewed the utility of pistons, which the ancient Greeks knew as well as the moderns, being one of the most simple inventions in hydraulics, as may be seen in Vitruvius, speaking of the pumps of Ctesibus for the forcing water to a great height,

In

In the memoirs of the Academy of Sciences at Paris, page 112, 1699, is to be seen an account of Mr. Amonton's, who shewed the drawing of an engine, in which fire heated air, which was to force up water on a wheel, to make it turn. We suppose he understood his theory; but to me it appears impracticable, though it must be said that Monsieur Amonton's notions are ingenious and useful for those who desire to know to what degree the expansion of air, for procuring power, can be brought.

Amonton's
rotatory engine,
1699.

There is also seen in the same Academy's works, "Mr. D'Alefmé 1705 D'Alefmé's proposal to raise water by means of steam, proceeding from hot water; in a machine (a sort of æolipile) in which the elasticity of the steam makes the water spout to a great height, so strong is that expansive power." August 5, 1705.

In 1707, the famous Papin published, at Cassel, a treatise on a new machine for raising water by means of fire; he thought his manner was better than Savery's. He says, "All the world shall know that it is the Landgrave of Hesse to whom this invention is due, and by him it was brought to the perfection it is in, though many obstacles were found in the execution."

Papin, 1707.

I cannot help remarking, what a pity it is that such an ingenious man as Papin should take so much pains to acquire the reputation of being the first in the invention of fire-machinery. To appear less partial, he takes from his own merit to flatter the landgrave, who certainly did not understand those matters so well as he: but, notwithstanding the consideration that is due to a sovereign and to the celebrated Papin, I make bold to say, that this engine was far from having the merit of Savery's; and I can say more, experience has made me believe that no man has seen the doctor's engine work a couple of hours, as may be conceived from the difficulty he found in making steam, and encreasing its elastic power in his receiver. Papin has nevertheless given us demonstration in upwards of forty pages of calculations, which prove his understanding and learning. This machine is in my academical works.

In 1710, Thomas Newcomen, ironmonger, and John Cawley, glazier, at Dartmouth, contrived to work a piston in a cylinder, by the weight of the atmosphere only, when they had formed a vacuum in their vessel; but it took them much

Newcomen and
Cawley's atmos-
pheric engine,
with a piston,
1710.

much time to bring such a complication of apparatus to perfection, for which they were criticised more out of jealousy than reason; however, they brought their engine to perform by hand as well as possible for that time, much to their honour, as well as to that of a country boy whose name was Humphry Potter, and who, to give himself time to play, contrived tackles to make the engine work itself.

The bringing this engine to perform as it did, proves that, when nature is ingenious, it finds out the way to come to its ends without the humdrum pedantry of schools. An iron-monger, a glazier, and a country boy, brought to perfection the lever fire-engine, which all the universities and academies of Europe had not the least notion of before these ingenious mortals appeared. This discovery has not been improved but in accessories, and by no means as to principles, as these are more simple, and can work with less fuel than those so much vaunted, if care is taken in making them.

As soon as these happy inventions came to light, Beighton, another ingenious man, slept forth and made tables for the dimensions of vessels, in proportion to the weight of the atmosphere, which proportions are followed to this day. This has made the world believe that the whole art of fire-machinery is concentrated in Newcomen and Cawley's lever-engine.

Defaguliers,
1718, and
Moura, their
improvements.

In 1718, Doctor Defaguliers made an amendment (according to him) on Savery's engine; but his method was not followed, as it caused the death of the manager; which must often happen when the dimensions are in the manner he contrived them.

After this engine, Mr. Moura, a Portuguese gentleman, invented some geers to make an engine, on Savery's principles, to work itself: they were ingenious, but too complicated to be put in practice.

The making a fire-engine for York-buildings gave an opportunity to improve as to the manual part, and to be less expensive.

Large cylinders, Experience in making fire-engines has brought Mr. Darby, of Colebrook-dale, to make the cylinders large enough to have a vacuum in them, equal to the weight of 80,000lb. of atmosphere, ten and fifteen times in a minute. This perfection of workmanship has procured to England a great ex-

portation

portation of cylinders, pipes, &c. to many parts of Europe and America. At Liege, and in the Netherlands, they used to have their cylinders from England; but, being very expensive from the duties and carriage, &c. the industrious Polson, at Liege, with his small foundry, found means to make cylinders of three pieces screwed together, which he bored, and made them serve as well as if they were of one piece. I have seen one work as well as can be desired.

In 1752, Monsieur Rivatz, a Swiss gentleman, made a fire-engine on Savery's principles for the Chevalier Nugue, at *Nogent sur Marne*, two leagues from Paris; the apparatus to make the engine work itself was of his invention, and performed very well; his boiler, however, being out of proportion, the man who attended the work was much hurt by the explosion of the steam, which discredited the machine, though well contrived in every part except the boiler.

 Next follows the Account of the Author's own Works.

XXII.

Observations and Communications on the Dry Rot in Timber, made to the Society for Encouragement of Arts.*

THE mischief arising in buildings from that decay of the timber and wood-work, known in general by the name of the Dry Rot, has been, and yet continues so great, as to demand every attention for its prevention. In the XIIth Volume of the Transactions of the Society of Arts, published in the year 1794, will be found some valuable facts, furnished by Robert Batson, Esq. of Limehouse, respecting the methods he took to prevent this evil, in one of his rooms greatly affected by it. The plan he adopted was, to charr the ends of his timbers, to take away the infected earth to the depth of two feet, and to fill up that space with anchor-smiths' ashes, or ashes from a foundry, before his flooring-boards were laid. On the 15th of May, 1794, which was upwards of six years after the flooring was laid, as above mentioned, a minute examination of the boards, wainscot, and timbers, was made in the presence of a Com-

Introductory remarks by the Society of Arts.

Robert Batson, Esq. his method of curing the dry rot.

* Extracted from their Transactions, Vol. XXI;

mittee of the Society, and they were all found entirely free from any appearance of the Rot. To investigate the matter more fully, a further inquiry has been made in June, 1803, and an answer received, that there has been yet no appearance of the Dry Rot there; the Society, therefore, think it may be of consequence to notice the fact, and to insert, in the present Volume, some other papers with which they have been favoured upon the subject. They contain many hints deserving public attention, and which will doubtless tend to check the progress of this evil.

Letter from BENJ. JOHNSON, Esq. on the same Subject.

TO CHARLES TAYLOR, Esq.

SIR,

Account of a
pew very much
affected by the
dry rot.

SOME time between 1771 and 1773, I went, at the request of a friend, to the Chapel at the Lock-Hospital, through curiosity, to examine a pew there, that had frequently been repaired for damages by the dry rot.

It is a plant.

After a close investigation, we found that it was the operation of a plant, whose leaf resembled that of the vine. Wherever it had touched, the effect of its poisonous quality got through the wood to the paint, which I have seen a mere skin. I proposed to cover the floor with bricks, laid in mortar, which was accordingly done. I called twice since, the last time about seven years ago; and have reason to think that it had never appeared again.

Destroyed by covering the
ground.

Dry rot at Mark-
Hall.

The next opportunity of examining it carefully was at Mark-Hall, in Essex, the seat of Mr. Montague Burgoyne. In a parlour there were three pillars of about ten inches in diameter, the outwood of which was between two and three inches thick. Two of them were eaten through in less than seven years, from the bases, about two feet upward, within the hollow, and were as rotten as if it had been the effect of a hundred years standing. Mr. Montague Burgoyne's gardener was a botanist: we found the plant where I directed him to search for it; and he said it was the *Boletus Lachrymans*.*

The plant found.

At

* Some authors call it a parasitical plant; and it is sometimes to be found with the willow and fallow tribe, but this is not to the purpose.

At another time, I saw it in a house at Whitehall, built by John Vanbrugh, whose nephew then lived in it. The house, I think, only two stories high; the plant had ascended to the upper story, committing devastation on the wainscot all the way. It will destroy half-inch deal wainscoting in a year.

Another instance at Whitehall,

I have had it twice in houses I inhabited, one in Suffolk, and other places, and the other in Gloucestershire. I bore with the first; in the other case, I undertook, and did stop it effectually.

The cause is from the floor being laid on the earth, which has been, where I have observed, of a gravelly or sandy loam. The moisture from a water-course at hand, or a North aspect, where the outer wall stands in a garden-bed, so that the rain percolates, are great encouragers: it requires moisture.

It is caused by communication with moist earth;

It never rises in the middle of the floor; because, if the seed were there, it could not germinate for want of air; but it is easy to suppose, that after the floor is shrunk, an air may be created between that, and the vacancy between the wainscot and outer wall sufficient for the purposes of vegetation.

with access of air.

I saw an instance, last summer, in the house of a friend, a student in botany. He was surprised when I told him, it was visit from a plant; but so it proved, and always is, and ever was so; nor does it originate from any other cause.

In my own case, I removed the original soil near the part affected, and supplied its place with sand. I then placed pieces of tile; on those I laid mortar, and tiles over them, pushing them under the wainscot, so that it had no communication with the joists or floor. Pillars, in like manner, should be kept from the earth.

Process of cure.

In laying a floor upon the ground, I should take away the earth for a foot in breadth, and four inches in depth, all round the walls, and place the ends of the joists in mortar, covering them with tiles pressed under the floor and wainscot, quite to the outward wall. Iron or tin plates would do, but are not so cheap as mortar and tiles.

Purpose. Till within a few months I have never been without some leaves of the plant. For many years they appear exhausted and dead, and soon crumble into dust; but I suspect that fresh wood attracts a fresh growth from the root.

This

The plant does not run except along wood.

This plant has no adhesive powers, but in contact with wood. If it could pass over brick or mortar, it might be seen to spring from the cellars, and infect half the houses in the kingdom.

In short, the wainscot is to be kept free from contact with the joists and floor; and I believe it cannot be better effected than I have described.

I am, SIR,

Your obedient Servant,

Dec. 21, 1799.

BENJAMIN JOHNSON.

To the Secretary.

(To be continued.)

SCIENTIFIC NEWS.

*Society of Agriculture, Commerce, and Arts, at Boulogne-sur-Mer.**

THE following questions proposed by this society are noticed as of general and not local import.

What are the nature of marles, their species, and the best modes of using them?

1. It is generally acknowledged, that marling is one of the most important means of improving land; accordingly the society will bestow a prize on the memoir, in which the nature of marles, their various species, and the most advantageous mode of using them, according to the difference of soils, shall be best unfolded.

Their external characters necessary to be known.

The author will be particularly careful to point out to farmers external characters, that will enable them easily to distinguish each species of marle.

Sheep liable to the vertigo or dunt. In France, 900,000, or 950,000, that is 3 or 4 in 100 die of it yearly.

2. Sheep are subject to a disorder known by the name of *vertigo*, or the *dunt*. The number that die of this disorder may be estimated without exaggeration, at three or four in a hundred annually. If then there be about thirty millions in France, we may reckon, that nine hundred, or nine hundred and fifty thousand are thus lost every year. It is occasioned by a hydatid formed in the head of the sheep. This hydatid

* *Magazin Encyclopedique*, No. 3. June, 1803. p. 401.

contains

contains an extremely limpid water, and a white substance, divided as if into grains, which have been found by the microscope, to be real worms. It appears, that to cure this disorder, the head of the sheep must necessarily be opened, and the hydatid extricated: but this operation has hitherto appeared so hazardous, and has been attended with so little success, even in the veterinary school at Alfort, that the Society has deemed it highly important to the science of agriculture, to offer a prize to the person who shall point out the best methods of treating the vertigo of sheep, and curing them perfectly of this disorder. He will take care that his method be supported by experiments made with accuracy, and confirmed by unquestionable testimony.

Caused by a hydatid in the head.

Its extraction dangerous.

What is the best method of treating it?

The prizes will consist of medals, which the society will deliver at the public meeting, on the 28th of April, 1804.

Crystallization of Lime by Tromsdorff, denied by Berthollet.

IN the first volume of the octavo series of this Journal, p. 302, is inserted "A method of crystallizing lime, by Professor Tromsdorff," translated from the Journal der Pharmacie, Vol. IX. part I. p. 108. The same process is also in Van Mons's Chemical Journal, No. II. According to which, muriate of lime is to be boiled with one fourth or even less of caustic lime, and the fluid is to be concentrated. Afterwards, by slow evaporation, long slender crystals of *caustic lime* are obtained, which are to be freed from the adhering muriate by ablution in alcohol.

Error respecting the crystallization of lime.

This result is denied by Berthollet, who, in his *Essai du Statique Chimique*, Vol. I. p. 350, says, "I have repeated this experiment, and have ascertained that these crystals were *not lime*, as has been announced, but *muriate of lime with an excess of lime*." He continues, "if these crystals be treated with water, other proportions are established; the part which dissolves is the muriate, which retains but a small excess of lime, and the portion which is not dissolved retains a greater excess of lime; by adding water, successive separations may be obtained, and the proportions will depend on the relation of the dissolving force to the resistance of the cohesion."

Conversion

Conversion of Pieces of Cloth impregnated with a Solution of Salt into Soap of Wool, by the Action of the Galvanic Pile.
By BRUGNATELLI.

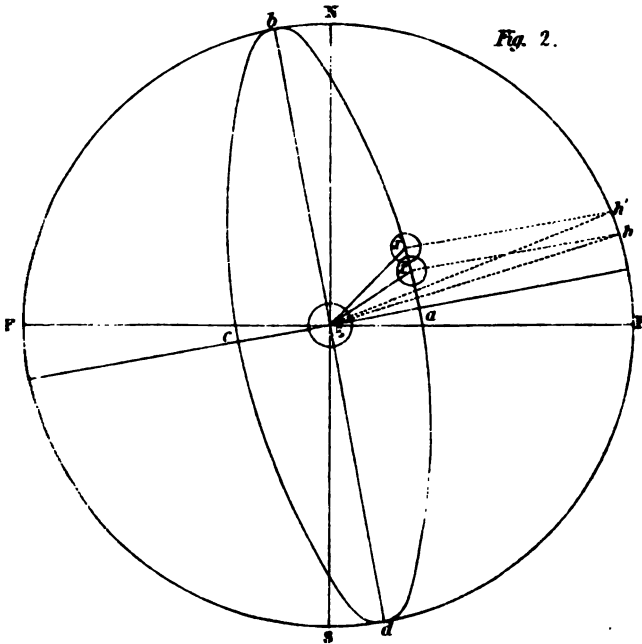
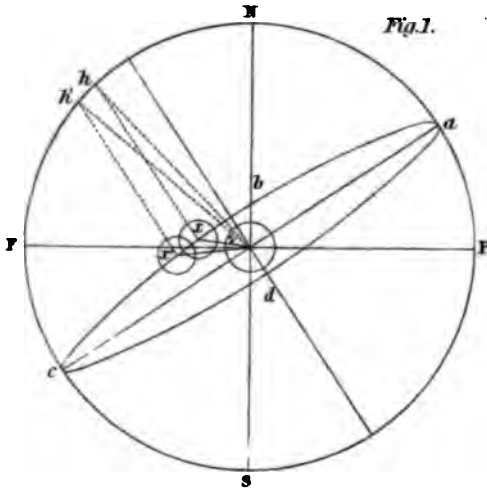
Cloth wetted with salt and water, used in the galvanic pile, is converted into soap of wool; the muriate being decomposed by the zinc, which is oxidized, and the soda combining with the wool.

I HAVE examined the singular alteration produced in the bits of cloth wetted with a solution of salt, interposed between the disks of copper and zinc arranged in the pile, and I have found, that they are at length converted entirely into soap of wool.

On this occasion the muriate of soda is decomposed by the zinc, that is continually undergoing oxidation, and the soda, separated by this decomposition, attacks the wool, and forms with it soap.

THE Work of Mr. Parkinson, of Hoxton, on the organic remains of the former world, is in considerable forwardness. The first part on the fossils of the vegetable kingdom, illustrated with coloured plates, in quarto, is proposed to be published on the first of June next.

*J. Herschels, Investigation of the Changes observed
in double Stars.*





*New Galvanic Apparatus for
combustion of Metals.*



THE N. Y.
PUBLIC L.

ASTOR, LENOX & TILDEN FOUNDATION
R

*Mr. Sharples's Apparatus for raising
Water by the fall of Waste Water.*





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OF
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CHEMISTRY,
AND
THE ARTS.

VOL. VIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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A
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THE ARTS.

MAY, 1804.

ARTICLE I.

*Letter from C. WILKINSON, Esq. on the Means of simplifying
and improving the Galvanic Apparatus.*

To Mr. NICHOLSON.

SIR,

THE communication from your ingenious correspondent *Introduction,*
L. R. I. afforded me considerable pleasure. As you favoured
me with his idea how the galvanic power may be increased
to an immense degree, prior to its appearance in your valuable
Journal, I have been induced to reflect upon various modes
of galvanic arrangements.

If the whole apparatus be made to consist only of a single *In apparatus of*
plate reflected backwards and forwards, so as to expose an *one single plate,*
immense surface, no plate of copper would be required, only *the coating of*
taking care, which in this instance would be easily effected, *copper is un-*
that one side of the plate only should be exposed to the action *necessary.*
of the acid. For, from various experiments, I am persuaded
that the other metal answers no other electrical purpose, than
to guard and protect the zinc side, to which it is soldered from
being acted upon by the acid. Even cement answers very *Cement will do*
well, if we only preserve through the cement a good con- *as well.*
ducting medium to the zinc. Thus with a zinc plate of eight
inches diameter, if only a piece of copper the size of a half-

VOL. VIII.—MAY, 1804.

B

penny

penny be soldered in its centre, and all the rest of that surface, which is usually soldered to copper or silver, be well covered with cement, equal effects would be obtained.

Proof in the Couronne de Taffes;

In a Couronne de Taffes I find the same effects are produced, whether the copper be of a corresponding size to the zinc plates, or, whether they be merely simple copper or silver wires.

and in a pile.

Upon this principle I have constructed a pile, and find it produces the same effects, as if the whole surface were covered with copper; I purpose soon to arrange a trough upon the same principles, and I am persuaded, that the more tedious and expensive part of galvanic apparatus may thus be prevented.

The corrosion in a trough is greatest near the air.

When a trough has been sometime employed, upon removing the plates, the loss of metal, I always observe, does not take place uniformly over the surface, but in the upper part, which in the galvanic action is the most exposed to the atmosphere, is the most acted on, and towards the bottom, the metal is very little altered. In order to preserve a more equal action, I am now preparing a battery formed of plates of ten inches by two and a half, the longest side placed horizontally, and I am persuaded, that this sized plate will produce more active effects than a plate of five inches square.

Hence a pile of dishes would be preferable.

It is well known, that in every galvanic operation, oxidation is produced, and if oxygen can be procured from the surrounding air, the effect would be more easy than producing it from the decomposition of water. I have no doubt, that if a series of zinc plates formed like large pewter dishes, were to be arranged in a pile-like form, insulated from each other, and the galvanic mixture to be poured in the hollow part; the lower side being covered with cement, excepting that from the centre a piece of copper should be soldered, and so projecting as to be in contact with the fluid in the cavity of the plate below; that a very powerful apparatus would thus easily be formed.

On the substitution of air instead of wet mediums by Dyckhoff.

In the latter part of your Journal I perused with a considerable degree of surprise, some observations of a Mr. Dyckhoff relative to the substitutions of thin strata of air, instead of wet media. As the results of his experiments appeared so contrary in principle to any I had tried, I immediately repeated them in the manner he has described, with interposed

lentils of glass, so as to render the separation very small. I then arranged several series of plates of one inch, to plates of seven inches in diameter; because he has not particularized the size of the plates he employed. I arranged piles of these different sizes to the number of twenty pairs in each arrangement, and observed not the slightest galvanic effect, either by my tongue or by any other test. I employed a very excellent condenser; no influence at all was evinced; and, lastly, I subjected it to the most delicate electroscope we have, viz. the muscular fibres of a frog. Not the slightest disturbance took place, although I fancy I have demonstrated, in the elements of galvanism I have published, that the sensibility of this animal electroscope is fifty thousand times greater than that of the condenser. The experiment did not succeed.

As to the charging of a Leyden phial, I am convinced it is perfectly erroneous to suppose it has ever been done. I have employed from fifty pair of plates to fifteen hundred, and never yet have produced any charge. Nor indeed could such be expected from the weak intensity of the state of electricity in galvanic operations; for a jar cannot be charged until a sufficient quantum is accumulated to overcome the resistance of the surrounding air, so absolutely requisite to the charging of a Leyden phial. Conviction of the author that galvanism cannot charge a Leyden jar.

I am, Sir,

Your's, &c.

C. WILKINSON.

ANNOTATION. W. N.

THE valuable observations in the preceding letter, will naturally suggest improvements to those who are employed in the construction of galvanic apparatus. It must be a great advantage, that the expence of copper and the work of soldering, or placing it, will be almost entirely saved. *Fig. 1. Plate III.* shews a simple method of disposing a single plate of zinc of large surface in a trough. It is supposed to have been made sufficiently hot to bed itself in cement at the bottom of the box in which it is placed, and its two ends A and B are secured in the same way. The shaded spaces represent the cavity occupied by acid, and the dotted space is left empty. Form of galvanic apparatus in a single piece,

B 2

A com-

A communication may be made by a copper wire from the wet or corroded surface to the opposite or dry surface, as shewn at A and C.

Another of many pieces acting as one.

As it may be difficult to bend a very long piece of laminated zinc, and to keep it flat, so as to give it a secure and close fixture in the cement, I should prefer a number of strait pieces screwed together with varnished pieces of wood between their extremities: every second piece being perforated on the acid side, to permit a free communication.

More particular account of the pile of dishes.

Mr. Wilkinson's pile of dishes promises to be very effectual, and of easy construction. I suppose the dishes to be of zinc, either cast in a metallic mould or stamped, and that each should be provided with three short copper legs, soft-soldered on; after which, the lower face of the dish should be well defended by varnish or cement, and also the lower ends, but not the sides of the copper legs. Thus prepared, they might be easily builded up, and charged by a proper funnel, and an appropriate measure for dealing out the acid.

Galvanic trough of one metal convertible into a single plate, by varying the communication.

In *Fig. 2.* I have ventured to offer the sketch of a trough, consisting of zinc plates only, principally because it appears capable of being as speedily charged with acid as another trough, and may with facility and at pleasure be used either as one single plate, or as the usual series of plates, acting in succession upon each other. Let A B represent a trough constructed after the manner of Cruickshank; excepting that the plates are of zinc only: and let the alternate shaded cells be charged with diluted acid, while the dotted cells continue empty. This may, without difficulty, be done at one pouring, by means of a channel in the wooden side of the trough.

Arrangement to produce the usual effect of a large plate.

Whether this trough, so charged, shall act as one extended galvanic surface, or as a continued series, will be governed by the copper wires of communication. If the former, then a long copper wire having branches descending from it into every one of the acid cells must be duly placed; and another wire parallel to, but not in contact with, the former, and having double branches descending into all the empty cells, so as to touch both the dry metallic surfaces, must also be placed. Whenever a communication is made between these two principal wires or conductors, the galvanic energy will pass through the medium of communication. I have not given a diagram, because this disposition is very obvious.

If the latter effect be required, namely, that the trough should act like a continued series, the connection of the troughs must be differently made. A number of clipping pieces or spring forks must be provided of copper wire, as represented by the curved lines *a, b, c, d, &c.* from each of which proceeds a third leg or branch, as seen in the figure. These are applied (most conveniently above, but) not so as to touch each other; the clipping part of each embracing the dry surfaces of a pair of the zinc pieces, which are acted upon by the same mass of acid, while the third leg is immersed in the acid of the next adjacent cell. Every one of their legs or branches is disposed towards the same region or part of space; by which means the acid of each compartment acting upon a pair of zinc plates, on one surface only, is made to communicate, by the interposition of copper, with the uncorroded side of the next pair in succession; and so on, exactly as in the common trough or pile.

Arrangement
for the effect of
a succession of
plates.

II.

Observations and Communications on the Dry Rot in Timber, made to the Society for the Encouragement of Arts.

(Concluded from p. 318 of Vol. VII.)

Second Letter from BENJAMIN JOHNSON, Esq.

SIR,

THE observations I sent yesterday were taken from different parts of my note-book, in haste, because the second Tuesday in December was past; for it was by accident I saw the advertisement on Saturday; but wishing not to be deficient in information, I trouble you again.

The leaves of the plant appearing exhausted and dead, is owing to their having imparted all their juices to the wood, which changed it to a fungus, and not to a powder, like rottenness from length of time.

A more full account of the plant that occasions the dry rot;

The *Boletus Lachrymans* is of the fungus tribe, and is one of the few that have leaves, as the miseltoe, &c.

Nothing is more easy than to prevent the damage from the plant. Besides what I said yesterday, I am positive that a tile laid

laid close along the walls round the room, would prevent the growth of the plant, even without mortar; and perhaps it is only necessary where the walls are next to the air.

and the method
of cure.

Charring the ends of the joists for a few inches, and charring the side of the wainscot at bottom, next to the walls would be sufficient; for the plant cannot adhere to any thing but wood, and that possessed of its natural juices, to a certain degree; so that I question if old dry oak would receive it.

All the white soft woods, as beech, poplars, and deals, are for a long time ready to receive it. Repairing the damage with fresh wood, without removing the earth and plant, is only feeding the evil.

The plant is of the creeping kind, and cannot rise two inches; so that wood in all cases, must be in contact with the earth to support it.

A fungus broader than the palm of one's hand, and an inch or more in thickness, is commonly seen at the bottom of an old post, on the surface of the earth; but it is not easy to discern whether the wood or the earth furnishes the matter; so true is the observation of Muller:—" *Dans l'étude de la nature, on peut nous comparer à de petits enfans qui commencent à ouvrir les yeux; nous voulons parler beaucoup, et nous ne faisons que bégayer.*"

I am, Sir,

Your most obedient Servant,

BENJAMIN JOHNSON.

Leipzig, Dec. 21, 1799,

To the Secretary.

N. B. The qualities of this plant are unknown to most English botanists, as appears from their publications; but they are known to the Germans, who have habitually used more wood in their buildings than we have.

Third Letter from the Same,

SIR,

Affured that the pursuits of the Society for the Encouragement of Arts, &c. aim at the full investigation of whatever they propose for the public benefit, I cannot persuade myself that I am troublesome in going a little further into this subject.

I had

I had lately a conversation with an old friend, who showed me two parcels of rotten wood, from an oak barn floor, laid about sixteen years ago. After lying twelve years it shook upon the joists. On examination, it was found to be rotted in various parts, and the planks, two inches and a half in thickness, were nearly eaten through, though the outside was glossy, and without blemish. The joists, and a large middle beam were laid at the ends, in brick and mortar, to create a firm level. No earth was near the wood; and he thinks that no air could find a passage. The rottenness was partly an impalpable powder, of the colour of Spanish snuff, and other parts were black, as if burnt; the rest was clearly a fungus.

This gentleman is a person of undoubted veracity; but a nice and exact observation is necessary in such examinations. He thought nothing of any plant, and it is likely there was none of the *Boletus*; so that my assertion that it was always to be found, was rather too systematic.

I asked him if the timber was dry when laid down. He could not however say that had been particularly adverted to. It had been sawed from a large oak, and was, as he thought, in all respects proper for a barn floor. As this seems not the operation of the *Boletus*, how did it happen?

We know that the oak, when in vegetation, is subject to what I shall call an exudation of juices, which produces a fungus, named the *Agaric* of the oak, with which the Druids of old played many tricks. The oak, then, if sawed into thick quantities, may emit these same juices, as the progressive course of nature to its entire decay.

We have all seen oaks of vast size and ancient record, with a great part of the outside whole, and all the inside gone; perhaps the work of a century. In all hollow trees fungus is discoverable. To use a law term, it is a *mishomer* to call it dry-rot; for the rotting principle is in moisture.

I had never seen the rot upon so large a scale as in timber, till lately. The prevention, then, of beams, rafters, large joists, and posts, put into the earth, from decay by the rot, is in charring only, which will dry up all the fungus juices of wood in large substance. Paint, or a bituminous preparation, may probably stop up the pores, and prevent the rot in slight work, where the treatment I before observed, with fire, might be incommodious, as in half-irish wainscot, &c.

The

Durability of charcoal.

The incorruptibility of charcoal is attested by undoubted historical facts, at the destruction of the famous temple at Ephesus. It was found to have been erected on piles that had been charred; and the charcoal in Herculaneum, after almost 2000 years, was entire and undiminished.

I am, Sir,

Your most obedient Servant,

BENJAMIN JOHNSON.

Ipswich, December 26.

Letter from RICHARD RAMSDEN BRAMLEY, Esq. of Leeds, relative to the Dry Rot in Timber.

To CHARLES TAYLOR, Esq.

SIR,

Introduction.

I take the liberty of inclosing to your care an Essay on the Dry Rot in Timber, which you will be so obliging as to lay before the Society for the Encouragement of Arts, &c. Should this Essay be deemed worthy of attention, or should any farther notice be necessary respecting it, every information that may promote the views of your respectable Society will be given with pleasure by,

Sir,

Your most obedient Servant,

R. RAMSDEN BRAMLEY.

Leeds, Aug. 26, 1799.

As the Society for the Encouragement of Arts, &c. have for some years offered a premium for the discovery of the cause occasioning the dry rot in timber, of which, it seems, no satisfactory account has yet been received; should the following prove so, it will give the author much pleasure. To bring the matter to the test by experiments, would require the observation of a long period, and in selected situations.

Wood, used for the general purposes of man, is cut down at different periods; and although it may be felled at the proper season, or when most free from sap or moisture, it is not always to be effected.

Even admitting it to have been cut down in the most favourable situation, it still abounds with such an extra proportion of

moisture

moisture, as to require a regular exposure to the air, prior to its being applied to use, if we wish to guard against that shrinking which always takes place, where this precaution has not been taken.

Facts and observations concerning the dry rot in timber, and its cure.

Although the fir kind contains less of this watery portion, yet it assuredly possesses a considerable share; and it is in this species, I apprehend, that the evil called the dry rot most generally occurs, as from the facility of working the same, it is most generally applied in buildings.

But supposing it to be fir, or any other species; wood felled when abounding with any extra proportion of sap, and applied to use without the proper seasoning or exposure to a free current of air, until such extra moisture as has had time to exhale, is most liable to the disease in question; and the cure, or principal prevention against it, would be the precaution of felling all wood *only* at the proper season, or when the sap is not in circulation. The next mode of prevention would be to use such wood *only* as has been for a considerable period exposed to the influence of a free current of air, or where convenience will admit, to that of air heated to a moderate degree; such air extracting with greater facility the inclosed moisture, and in a more certain ratio than the irregularity of our atmosphere will allow.

In all rapidly-improving countries, this evil is likely to be an increasing one, as the current demand for wood generally exceeds the supplies laid by in store, so as to be applied to use in regular succession, after being properly seasoned.

Another cause that affects all wood most materially, when not fully dried, is the application of paint, the nature of which prevents all exhalation, and confines the inclosed moisture, till it occasions a fermentation through the whole fibrous system of the wood, and brings on a premature state of decomposition, or the dry rot.

A similar evil may be induced, in consequence of any newly-finished building having all the doors and windows shut up, and that for some length of time, particularly in moist weather. The wood, even though unpainted, is thus frequently placed in an atmosphere more charged with vapour than its own internal contents, and is consequently in an imbibing instead of an exhaling state, and tending to decay. Wood placed in damp situations, and the ends of timbers near to moist walls, suffer from similar causes.

What

Facts and observations concerning the dry rot in timber, and its cure.

What particularly attracted my observation to the circumstances was this, that both oak and fir posts were brought into this premature state of decay, from their having been painted prior to the due evaporation of their moisture; and then extending the observation, and tracing the history of other wood affected in a similar manner, I am convinced that the evil frequently thus originates, and its prevention would be in using timber, previously well dried and seasoned.

RICHARD RAMSDEN BRAMLEY.

SIR,

A considerable time has elapsed since I furnished you with some observations relative to the dry rot in timber, and having been since engaged busily in draining from 4 to 5000 acres of ground, further ideas on the subject of the dry rot have in the interim recurred to me from the work I have been engaged in, which, if the respectable Society to which you are Secretary think worthy attention, they may add to, or connect with my former ideas, as may be deemed most useful. Where houses are troubled with damp walls, near to the earth's surface, it is generally, if not universally, occasioned by the percolation of water from the higher adjoining ground, which, thus intercepted in its current, attempts to follow the general hydrostatic law, of elevating itself, by the syphon line, to a height equal to that from whence it has its origin. Thus, in houses differently situated, we see the damp arising, to varying degrees of height, on the walls; and those are probably all corresponding to the height at which the moisture circulates in the adjoining ground. At its first entrance to the building, and whilst the moisture is in small quantity, the excavated part of the foundation wall may absorb, and gradually quit such proportion; but the excess, as is generally the case in moist weather, exceeding that power, the foundation stones are then saturated in a more rapid proportion than the adjoining rarified internal atmosphere can evaporate; the watery particles then creep up, in degrees proportionate to the ascent from which they originally descended, excepting when prevented, or driven off by the superior heat of the adjoining rooms, when, in addition to the disagreeable damp they cause, they frequently occasion considerable damage to pictures, furniture, &c. Drains laid out athwart the ascending ground, with a
very

very slight descent or fall, and made of the depth of one yard ^{Facts and obser-} for each yard of ascent, and from the foundation until equal ^{vations concern-} to the height that such damp ever rises, would, there is little ^{ing the dry rot} doubt, completely secure the house and furniture from the ^{in timber, and} inconveniences hitherto sustained, and would generally prove ^{its cure.} an effectual prevention to most cases of the dry rot, where it originates in extreme moisture. I am of opinion that the fungus which pervades decaying wood is not the first cause, but an attendant on the peculiar state to which such wood has been reduced by prior causes. The disseminated seeds finding a proper bed, or *nidus*, like the mushroom, toad-stool, &c. fix there their abode, and pervade the whole substance, thus accelerating the general law of Providence, which tends to make all matter re-productive.

Cellars; or such other places, should be drained in the manner I have above mentioned, by taking off the percolating water, prior to its gaining admission to or contact with the walls; and it is probable that, in most cases, a single drain will have complete effect; it would assuredly do so, if it was not for the variation of the earth's internal strata, which are not easily discernible. If attention to this rule was paid prior to the building any new streets in towns, it would prove essentially useful.

I am, with esteem,

DEAR SIR,

Your's truly,

Leeds, June, 1803.

R. RAMSDEN BRAMLEY.

To Mr. Charles Taylor.

The Society have been informed, that mortar made of lime from burnt chalk is much more destructive to timber than stone lime, or that burnt from lime-stone. Chalk lime attracts moisture; and communicating it to any timber which it touches, occasions its decay.

Sea sand is also prejudicial, if made into mortar, from a similar quality of attracting moisture from the atmosphere: this may in some degree be corrected by washing the sand well in fresh water, where good sand cannot be procured.

Good mortar, where any is required to be in contact with timber, may be made from a mixture of stone lime fresh burnt, and

and river sand, to which a very small quantity of common brown, or yellow iron ochre, should be added, and well incorporated therewith.

III.

On the Figure of the Earth. By PEREGRINUS PROTEUS.

To Mr. NICHOLSON.

SIR,

IN some of your late Journals I observe a paper on the figure of the earth, by Mr. John Playfair, professor of mathematics in the University of Edinburgh, containing several new theorems, and ingenious remarks, on a subject which has engaged the attention of the first mathematicians of Europe since the days of Newton. On reading it, I was led to examine the properties of spheroidal triangles, and to investigate the problem, proposed by the author, for determining the dimensions of the earth from the length of the straight line or chord joining two places whose geographical situations are given. These are intended to form the principal subject of this letter; but, before I proceed to them, I beg leave to make a few observations on that paper, without any view to cavil, or detract from its real merits.

Observations
upon Professor
Playfair's
memoir on the
figure of the
earth.

After taking notice of the disagreement in the compressions of the terrestrial spheroid, which result from the comparison of different measurements, he assigns, as the principal reason for this inconsistency, the local irregularities in the direction of gravity, arising in some situations from the attraction of mountains, and in others from the unequal density of the materials under, and not far from, the surface of the earth. That the first has a sensible effect on the plumb-line has been proved by accurate and undeniable experiments; the second is an ingenious and probable conjecture, which the surveys carrying on in Great Britain and France will afford data to refute or confirm. But though the former may operate in the general survey of a country, where the observer has not his choice of ground, it has always been avoided as much as possible in measurements made for the express purpose of determining the figure of the earth; and though the latter may produce some perceptible difference

difference in observations made in nearly the same latitudes, does the author think it sufficient to account for the great disagreement in the results from the comparison of distant observations? Is it not much more probable, without giving up the elliptic figure, that some of the observers may have used different standard measures from the rest, or not made proper allowances for the alteration of their lengths in different temperatures? In short, this circumstance appears to me sufficient to account for some small local irregularities, but wholly inadequate to explain the great differences in the general results.

Observations upon Professor Playfair's memoir on the figure of the earth.

The author then proceeds to point out several methods of calculating the dimensions of the earth from terrestrial measurements. The first applies to the case where two arches of the meridian are given in different latitudes, which, under the most favourable circumstances, is incomparably the most accurate that can be employed. The rules he gives are certainly very simple, and in some respects new; but he seems to be mistaken when he asserts, that the calculation must be made by rules quite different from those that have been hitherto given. Euler's * is essentially the same with his own; and Du Séjour, Legendre, Delambre, &c. have given many accurate theorems, which may be applied to this purpose. The second method is, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the same latitude; and the third from the measures of degrees of the curve perpendicular to the meridian in different latitudes. His theorems for both are very accurate and simple. But the principal novelty of Mr. Playfair's paper is, the method he proposes of finding the figure of the earth from the length of a straight line or chord joining two places whose geographical situations are given. As he has left the solution of this problem to some future occasion, the following perhaps may not be unacceptable:

Let PAO (*Plate III. Fig. 1.*) represent one quarter of the ellipsis, by the revolution of which round the semi-conjugate axis PC, half the terrestrial spheroid is generated. Let C be the center of the earth, P the pole, CO the radius of the equator = a , CP half the polar axis = b , and c = the compression at the poles, or the excess of a above b . Let A in the

Solution of the problem for finding the figure of the earth, from the length of a chord joining two known places, &c.

* *Memoires de L'Academie Royale des Sciences Belles Lettres a Berlin, 1753.*

meridian

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

meridian PAO be one of the extremities of the measured chord, and B in the meridian PB the other extremity; let AD, BF be drawn perpendicular to CP, BE perpendicular to the plane CPAO, and let AB, AE, FE be joined. Then will $AE^2 = (CD - CF)^2 + (AD - FE)^2 = CD^2 + AD^2 \times CF^2 + FE^2 - 2CD \times CF - 2AD \times FE$, and $AB^2 = AE^2 + BE^2 = CA^2 + CB^2 - 2CD \times CF - 2AD \times FE$.

Now let λ, ϕ be the latitudes of A and B expressed in decimals of the radius 1, ω the difference of longitude or the angle BFE, and D the length of the measured chord = AB. Then from the properties of the ellipsis we have

$$CA^2 = \frac{a^2 \cos. \lambda^2 + b^2 \sin. \lambda^2}{a^2 \cos. \lambda^2 + b^2 \sin. \lambda^2} = a^2 - 2ac \sin \lambda^2$$

$$AD = \frac{a^2 \cos. \lambda}{\sqrt{(a^2 \cos. \lambda^2 + b^2 \sin. \lambda^2)}} = a \cos. \lambda + c \cos. \lambda \sin. \lambda^2$$

$$\text{and } CD = \frac{b^2 \sin. \lambda}{\sqrt{(a^2 \cos. \lambda^2 + b^2 \sin. \lambda^2)}} = a \sin. \lambda - c \sin. \lambda$$

$(2 - \sin. \lambda^2)$, neglecting the powers of c higher than the first, because c is very small in comparison of a . Whence by substitution, and putting $\delta^2 = 2a^2 (1 - \sin. \phi \sin. \lambda - \cos. \phi \cos. \lambda \cos. \omega)$, we obtain the following equation;

$$\delta^2 - c \left(2a (\sin. \lambda - \sin. \phi)^2 - \frac{\delta^2}{a} (\sin. \lambda^2 + \sin. \phi^2) \right) = D^2,$$

and by extracting the square root of each side, and rejecting the square, cube, &c. of c , there results,

$$\delta - c \left\{ \frac{2a}{\delta} (\sin. \lambda - \sin. \phi)^2 - \frac{\delta}{2a} (\sin. \lambda^2 + \sin. \phi^2) \right\} = D,$$

$$\text{or } a + c \left\{ \frac{1}{2} (\sin. \lambda^2 + \sin. \phi^2) - \frac{2a^2}{\delta^2} (\sin. \lambda - \sin. \phi)^2 \right\} = \frac{D a}{\delta}$$

This equation may be otherwise expressed thus; let a spherical triangle be constructed, having two sides equal to the polar distances of A, B, and contained angle = their difference of longitude; whence find the third side, which put = ϑ . Then will $\sin. \phi \sin. \lambda + \cos. \phi \cos. \lambda \times \cos. \omega = \cos. \vartheta$, and $1 - \sin. \phi \sin. \lambda - \cos. \phi \cos. \lambda \cos. \omega = 1 - \cos. \vartheta = 2 \sin. \frac{1}{2} \vartheta$;

therefore $\delta = 2a \sin. \frac{1}{2} \vartheta$, and $D = 2a \sin. \frac{1}{2} \vartheta + c \left\{ \sin. \frac{1}{2} \vartheta (\sin. \lambda^2 \times \sin. \phi^2) - \frac{(\sin. \lambda - \sin. \phi)^2}{\sin. \frac{1}{2} \vartheta} \right\}$. — The value of δ

is manifestly equal to the length of a straight line joining two places, whose latitudes are λ, ϕ , and difference of longitude ω , on a sphere, whose radius is a .

From

From this equation the following method of determining the figure of the earth is deduced. Let l be the length of a measured chord, and λ, ϕ, ω the latitudes and difference of longitude of its extremities; find S as above, and let $m = 2 \sin. \frac{1}{2} S$, and $n = \sin. \frac{1}{2} S (\sin. \lambda^2 + \sin. \phi^2) - \frac{(\sin. \lambda - \sin. \phi)^2}{\sin. \frac{1}{2} S}$.

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

Then if we reject all the powers of c higher than the first, we shall have the simple equation $ma + nc = l$. In like manner find a similar equation $m'a + n'c = l'$, corresponding to any other chord whose length is l' , and there will result $a = \frac{n'l - nl'}{nn' - m'n}$, and $c = \frac{m'l - ml'}{mn' - m'n}$. The approximation may be easily carried further by including the second power of c , and thus finding an equation of the form $ma + nc + pc^2 = l$; but this labour would be useless, as the method itself does not admit of greater accuracy. If $\phi = \lambda$ the equation becomes

$\frac{D}{2 \sin. \frac{1}{2} S} = a + c \sin. \phi^2$, as is found by Mr. Playfair in § 31.

From the first equation a rule may be easily derived for calculating the difference of longitude of two places, when their latitudes and distance are given. For by transposition we have

$$D^2 (1 + \frac{c}{a} (\sin. \lambda^2 + \sin. \phi^2)) = D^2 + 4ac (\sin. \lambda - \sin. \phi)^2,$$

and by division, and rejecting the powers of c higher than the

$$\text{first } D^2 = D^2 + \frac{c}{a} (4a^2 (\sin. \lambda - \sin. \phi)^2 - D^2 (\sin. \lambda^2 + \sin. \phi^2));$$

but D^2 is $= 2a^2 (1 - \sin. \lambda \sin. \phi - \cos. \lambda \cos. \phi \cos. \omega)$, there-

$$\text{fore } \cos. \omega = \frac{1 - \frac{D^2}{2a^2} - \sin. \lambda \sin. \phi}{\cos. \lambda \cos. \phi} - \frac{c}{a} \left(2 (\sin. \lambda - \sin. \phi)^2 - \right.$$

$$\left. \frac{D^2}{2a^2} (\sin. \lambda^2 + \sin. \phi^2) \right) \text{ and putting } 1 - \frac{D^2}{2a^2} - \sin. \lambda \sin. \phi$$

$$= \cos. \omega' \text{ we have } = \omega' + \frac{c}{a} \times \frac{2 (\sin. \lambda - \sin. \phi)^2 - \frac{D^2}{a^2}}{\sin. \omega'}$$

$(\sin. \lambda^2 + \sin. \phi^2)$, which rule may be thus expressed. Let there be a spherical triangle, having two of its sides equal to the polar distances of the places, and the third side d such that

$\sin.$

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

$\sin. \frac{1}{2} d = \frac{D}{2a}$: find the angle ω' contained between the polar distances, and the difference of longitude ω will be $\omega' + \frac{2c}{a} \times \frac{\cos. \frac{1}{2} d^2 (\sin. \lambda^2 + \sin. \phi^2) - 2 \sin. \lambda \sin. \phi}{\sin. \omega'}$.

The latitude ϕ may also be found from the same equation, when λ , ω and D are given. For if the base of a spherical triangle be $= d$, the two other sides $= 90^\circ - \lambda$, $90^\circ - \phi$, and the contained angle $= \omega$, the $\cos. \omega$ will be $=$

$$\frac{1 - \frac{D^2}{2a^2} - \sin. \lambda \sin. \phi}{\cos. \lambda \cos. \phi'}. \text{ Now let } \phi = \phi' + x, \text{ where } x \text{ may}$$

be very small, and $\cos. \lambda \cos. \phi'$ there results $\cos. \omega = \cos. \omega' + \frac{(\cos. \lambda \sin. \phi' \cos. \omega - \sin. \lambda \cos. \phi')}{\cos. \lambda \cos. \phi'} \times \sin. x$: consequently

$$\frac{(\cos. \lambda \sin. \phi' \cos. \omega - \sin. \lambda \cos. \phi')}{\cos. \lambda \cos. \phi'} \times \sin. x = \frac{c}{a}$$

$$\left(2 (\sin. \lambda - \sin. \phi')^2 - \frac{D^2}{2a^2} (\sin. \lambda^2 + \sin. \phi'^2) \right) \text{ nearly,}$$

$$\text{and } x = \frac{c}{a} \times$$

$$\frac{\cos. \lambda \cos. \phi' \left(2 (\sin. \lambda - \sin. \phi')^2 - \frac{D^2}{2a^2} (\sin. \lambda^2 + \sin. \phi'^2) \right)}{\cos. \lambda \sin. \phi' \cos. \omega - \sin. \lambda \cos. \phi'}.$$

From the investigation of Mr. Playfair's problem, therefore, we have obtained very accurate rules for finding ω from λ , ϕ and D , and ϕ from λ , ω , D .

Now in order to find an equation expressing the relation between the latitudes, difference of longitude, and one of the azimuths, let AL be perpendicular to the meridian PAO in A meeting FE in L , and BK perpendicular to AL . Join KE , and the angle BKE will be equal to the spheroidal angle OAB , and BFE equal to the angle APB or difference of longitude. Let $OAB = BKE = A$, $BFE = \omega$, and λ , ϕ as before, then will LE be $= (CD - CF) \cotang. \lambda + FE - AD$, $KE = (CD - CF) \cos. \lambda + (FE - AD) \sin. \lambda$, and $\cotang. A = \frac{KE}{BE}$.

Whence by substituting the values of CD , CF , AD , BF given above, and rejecting the powers of c higher than the first, there results

results $\cot. A = \frac{\cos. \phi \sin. \lambda \cos. \omega - \sin. \phi \cos. \lambda}{\cos. \phi \sin. \omega} - \frac{2c}{a} +$
 $\frac{(\sin. \lambda - \sin. \phi) \cos. \lambda}{\cos. \phi \sin. \omega}$. But if ω be the vertical angle of a

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

spherical triangle, and $90^\circ - \phi$, $90^\circ - \lambda$ the sides; also A' the supplement of the angle opposite the side $90^\circ - \phi$; then will $\cot. A' = \frac{\cos. \phi \sin. \lambda \cos. \omega - \sin. \phi \cos. \lambda}{\cos. \phi \sin. \omega}$, and conse-

quently $\cot. A = \cot. A' - \frac{2c}{a} + \frac{\cos. \lambda (\sin. \lambda - \sin. \phi)}{\cos. \phi \cos. \omega}$,

Whence as A , A' are nearly equal, we obtain $A = A' + \frac{c}{a}$

$2 \sin. A'^2 \times \frac{\cos. \lambda (\sin. \lambda - \sin. \phi)}{\cos. \phi \cos. \omega}$, which will be found

abundantly accurate in practice, but if the square of c be re-

tained, and $\frac{2 \sin. A'^2 \cos. \lambda (\sin. \lambda - \sin. \phi)}{\cos. \phi \sin. \omega}$ be put $= M$,

and $\sin. A'^2 \cos. \lambda \times \frac{\sin. \lambda \cos. 2\lambda + 2 \sin. \lambda \sin. \phi^2 - \sin. \phi}{\cos. \phi \sin. \omega}$

$-\cot. A \times M^2 = N$, A will be $= A' + M \times \frac{c}{a} - N \times \frac{c^2}{a^2}$

more accurately. The rule may be thus expressed; let the colatitudes of the two places, and their difference of longitude form the sides, and contained angle of a spherical triangle, of which find the base angle at the place whose latitude is λ , and let it be $= \alpha'$, and the corresponding angle α of the spheroidal triangle will be $= \alpha' - \frac{c}{a} \times 2 \sin. \alpha'^2 \times$

$\frac{\cos. \lambda (\sin. \lambda - \sin. \phi)}{\cos. \phi \cos. \omega}$. In like manner if β' be the angle of

the spherical triangle at the place whose latitude is ϕ , the corresponding angle β of the spheroidal triangle will be found to

be $= \beta' - \frac{c}{a} \times 2 \sin. \beta'^2 \times \frac{\cos. \phi (\sin. \phi - \sin. \lambda)}{\cos. \lambda \cos. \omega}$. Con-

sequently $\alpha + \beta$ is $= \alpha' + \beta' - \frac{c}{a} \times \frac{\sin. \lambda - \sin. \phi}{\cos. \phi \cos. \omega} \times$

$2 \sin. \alpha'^2 \cos. \lambda - \frac{c}{a} \times 2 \sin. \beta'^2 \times \frac{\cos. \phi (\sin. \phi - \sin. \lambda)}{\cos. \lambda \cos. \omega} =$

$\alpha' + \beta' - \frac{c}{a} \times$

$\frac{2 (\sin. \alpha'^2 \cos. \lambda^2 - \sin. \beta'^2 \cos. \phi^2) (\sin. \lambda - \sin. \phi)}{\cos. \lambda \cos. \phi \cos. \omega}$; but by

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spherics $\sin. \alpha' \cos. \lambda = \sin. \beta' \cos. \phi$, therefore $\alpha + \beta = \alpha' + \beta'$ if we reject the powers of c higher than the first, which are insensible. Hence the principle laid down by Mr. Dalby, viz. that in a spheroidal triangle, of which the angle at the pole and the two sides are given, the sum of the angles at the base is the same as in a spherical triangle, having the same sides, and the same vertical angle, is verified, and therefore the concluding remark of Mr. Playfair is hasty and ungrounded. But perhaps Mr. P. in his solution retains the second power of c , and objects to Mr. Dalby's principle because its coefficient does not vanish except in particular cases. If so, the objection is frivolous, as the difference is so small as scarcely to be computed in the cases that occur in practice, and too small in any case to lead into error or deserve attention.

The preceding theorems for the solution of spheroidal triangles will be found extremely accurate, when applied to such as are described on the surface of the earth, on account of the smallness of c in comparison of a ; and in like manner others may be deduced, when different parts of the triangle are supposed given. Thus if λ , α and D be given; let a spherical triangle be constructed with one side $= 90^\circ - \lambda$, another $= d$,

such that $\sin. \frac{1}{2} d = \frac{D}{2a}$, and the contained angle $= \alpha$; find the other side $90 - \lambda'$, the angle at the pole α' , the other azimuth β' and we shall have equations of this form $\phi = \phi' + Pc$, $\alpha = \alpha' + 2c$, and $\beta = \beta' + Rc$, where P , 2 , R are functions of λ , α , D , which may be derived from the foregoing equations by proper artifices. But the formulæ, except in particular cases, will not be found so simple as the former. These, however, and some new theorems applicable to trigonometrical surveys, I shall delay to some future communication. In the mean time, it may not be foreign to the subject to remark, that the arch of the meridian, said to have been lately measured in the Myfore country in the East Indies, by Brigadier Major Lambton, gives the degree, in latitude $12^\circ.32'N$. equal to 60494 fathoms; which compared with that of 60795 in latitude $47^\circ.24'N$. gives $\frac{1}{180}$ for the compression at the poles, a quantity differing very little from the mean deduced from all the measures of degrees. But it must be confessed that there appear at present to be two very important objections against the accuracy of Major Lambton's measure. The Myfore, on account

Arch of the meridian measured in the Myfore country,

Account of the irregularity of its surface and its uncertain elevation above the level of the sea, is an unfit country for ascertaining a nice point of this kind, however well situated for connecting the eastern and western sides of the peninsula by a geographical survey; and the Major, from his account in the 7th volume of the Asiatic Researches, seems to be somewhat doubtful of the exact length of his chain. Nevertheless it is probably to India that we must look for the means of finally deciding this long contested question. There, and there only, we find many tracts of country highly favourable to this purpose; and it is to be hoped that the East India Company, while anxious to ascertain the extent of its possessions, will not entirely neglect the interests of science.

I am, Sir, &c.

PERIGRINUS PROTEUS.

Portsmouth, April 7, 1804.

IV.

Description and Drawing of a Hand Drill for sowing Peas, Beans, &c. Communicated to the Society of Arts, by the Inventor, Mr. ROBERT GREEN, of Westwratt, Cambridgehire.*

TO CHARLES TAYLOR, Esq.

SIR,

I HAVE invented an engine to sow peas, with which I have sown all my peas, to the amount of 40 acres, at the price of 1s. per acre, and think that my peas are much better than those sown any other way. It is also on a very simple plan, and the expence of it when complete is not 2l. It is used by manual labour, without any horse; and it will draw the drill, sow the peas, and cover them at the same time, and will sow them much rounder than any other I have yet seen. I likewise find I can do it much cheaper than with any horse, and am of opi-

* To whom the silver medal and ten guineas were voted by the Society. A complete machine is placed in the repository of the Society.

Very economical drill for sowing peas, nion that it sows much better than any drill I have seen. If the Society wish it, I will send a model for their inspection.

I am, Sir,

Your obedient Servant,

ROBERT GREEN.

Westwratting, Cambridgehire,

June 27, 1802.

SIR,

I HAVE sent the engine for sowing peas, in order that it may be laid before the Society for the Encouragement of Arts, &c. I intended to have sent a model of it, but afterwards thought that the engine itself would be more acceptable to the Society. I made it myself, and have sown with it 26 acres of land in my own occupation. Mr. Piper, a near neighbour of mine, has sown with it five acres; and Mr. Cock, of Blunt's Hall, Wratting, in Suffolk, 25 acres, at the expence of 1s. per acre. Several other gentlemen had drills of me for sowing peas. If I give my men 1s 6d. per acre, they will sow for me two acres in one day. I can with my own hand sow one acre in five hours, and at the same time sow the peas, draw the drill, and cover them, and make full twelve drills and a half to the rod. I likewise produce the plant much handsomer than any other seen in our country, and at a very trifling expence. By this too, the labour of horses is spared, which we find to be a very material circumstance. It will be a most excellent engine for gardeners in the neighbourhood of London; for I will be bold to say, that no man can sow with his hand, so as to equal this, at a very trifling expence.

I have spent much time in making implements of husbandry, but have made none so useful as this; for it is simple in its construction, may be purchased by any man, the expence being so trifling, and saves the labour of horses.

I remain, Sir,

Your most obedient Servant,

Westwratting.

ROBERT GREEN.

Description

Description of the Engraving of Mr. ROBERT GREEN's Hand-Drill, for sowing Peas, Beans, &c. Plate II. Very economical drill for sowing peas.

Fig. 3. a a, The wheels placed upon a wooden axis *b*, which is square at each end, but round in the centre. The square ends of the axle have holes throughout them, at different distances in order to deposit the seed at nearer or more distant intervals, as may be wanted.

c, The box in which the seed is placed: the axis *b* is cylindrical, and has holes made therein proper to receive the seeds, which by the revolution of the axis are carried forwards, and fall through an iron tube into the ground opened for them by the share *d*. When deposited in the ground, they are covered, or the earth drawn over them by two iron pins or scrapers *e*, fixed on each side of the tube, and extending some inches behind it.

ff, The handles of the drill-machine, by which it is pushed forwards.

Fig. 4. Shows an enlarged view of the interior of the seed-box *c*, above mentioned, and holes for the seeds placed in a spiral line, in order to drop the seeds more regularly.

g, Is a small brush within the box, which rubs against the cylinder, to keep the holes clear to receive the seeds.

Fig. 5. Is a section of the machine, where *a* is part of the seed-box; *b*, the round part of the axle, which delivers the seed.

d, The share which opens the earth.

h, The tube through which the seed falls.

i, The mouth of the tube, and one of the fins which draws together the soil, and covers the seed.

k, Is a small door, to be opened occasionally, if the roller or tube are out of order.

l, A strong flat board, to which the iron work is screwed.

Fig. 6. Shows an enlarged plan of the iron work, when the machine is reversed.

d, Is the share.

i, The hole from which the seed is dropped.

e e, The two fins, or scrapers, which collect the earth and cover the seed.

N. B.

*Enquiries concerning the Methods of investigating the
Velocities of a Body supposed to be projected from the
the Earth. By a Correspondent.*

To Mr. NICHOLSON:

SIR,

Theory of La
Place, that
stones fall from
the moon.

AMONG the various theories concerning the stones
upon the earth in different parts of our globe, (the subject of
Mr. Howard's Analysis), that which De la Place has
upon, though apparently incapable of proof, seems to be
improbable, namely, that they are projections from
volcanos. Surely, Sir, whether this is or is not possible
be mathematically demonstrated, certain data being
If you, or any other able mathematician, could find
do this, it would be a great gratification to see the solution of
these questions.

Data for computation.

We must assume as data, That the density of the matter
composing our satellite, is precisely similar to the density of
our globe; and that the moon has no atmosphere to
projection from its surface, or so small a one as not to be
calculated upon, since, probably, it is rare and low, per-
haps more than one-sixth of a mile high.

Now their relative bulks and distances are pretty well
known, and we will take them to be precisely known.

Their comparative centrifugal forces may easily be
calculated, as the moon revolves about 27 times slower
about its axis than the earth about hers.

Let us suppose too, that this body weighs one hundred weight.

What will be the velocity requisite to overcome the mutual attractions of the moon and this body, so as to project it beyond their powers; remembering that its velocity will be continually diminishing as long as any attracting power acts upon the projectile, and calculating the aid it would receive from the centrifugal force. Results to be investigated.

In the journey of 240,000 miles through which it has to travel in a direct line, is there any free space beyond the sphere of the moon's attraction and that of our planet's? If so, at what distance from the moon will that be found? and at what distance from the earth? and with what velocity may it be supposed to travel through that space? of course it will be much slower than when first projected.

As upon entering the limits of the earth's attraction its velocity will be again increased, *quæ* its rate of travelling to the earth; and, taking the three reckonings into account, in how many days and hours can it be mathematically demonstrated that it would reach our solid globe?

Acted upon by the united forces of projection, centrifuge, (if I may coin a word) and the motion of the moon in its orbit, and the force of attraction and orbital motion of the earth, what will be the precise line it may be presumed to describe in its course?

Would it not have a revolving motion during some part of its course?

What are the calculations by which we may be enabled to judge that 3 or 5 times the velocity of a cannon-ball, at the moment of projection, would enable it to counteract these impediments?

It has surprized me, that the numerous late publications mentioning this theory, have not detailed the mathematic processes by which it seems capable of being solved, or of proving its fallacy.

I hope, Sir, you will not think this obtrusion impertinent. You obligingly and satisfactorily complied with my request in a note concerning Col. Blaquiere's gun to throw double headed shot; this has emboldened me to express my wishes on the subject of this letter. But I do not wish to have my ignorant questions
load

THREE-FURROW PLOUGH.

load your valuable Journal, though I shall feel myself much obliged by any further information upon this head.

I remain, Sir,

Your most humble servant,

A CONSTANT READER.

VI.

*Drawing and Description of a Three-Furrow Plough. By the Rev. EDMUND CARTWRIGHT, of Woburn, Bedfordshire.**

To CHARLES TAYLOR, Esq.

DEAR SIR,

Economical
three-furrow
plough.

I ENCLOSE you a certificate of the performance of a plough of my invention, which has occasionally been at work through the whole summer. For this last fortnight, it has been used for ploughing in wheat under furrow. Though a very useful instrument at all times, it is particularly valuable at the seed times, and the turnip season; because at those times it frequently happens you lose the most favourable opportunities, for want of ability to execute your operations with sufficient dispatch.

Weekly saving
4l. 16s.

I need not calculate to you the saving on the use of this plough. It is worked (on light land I mean) with a pair of horses, without a driver. A pair of horses and a ploughman cannot be laid at less than 8s. per day. As two sets of these are saved, the weekly saving by the use of this plough amounts to no less than 4l. 16s.

Applicable to
level lands in a
tolerable state of
cultivation.

Useful, however, as I find this instrument on our light level lands, I am not so partial to it, to suppose it is equally calculated for all soils, or all kinds of ground. For instance, where the ground is very uneven, or the ridges are narrow and steep, I would not use it; neither when the land is very foul with root weeds. In all these cases a single plough is certainly to be preferred: but in all cases where the ground is in a tolerable state of cultivation, and where it lies reasonably level, it will be found a most valuable acquisition.

* From the Transactions of the Society of Arts, who voted him the silver medal. A model is placed in their repository.

I will

I will thank you to communicate this letter, and the certificate accompanying it, to the Committee of Agriculture; and and if they are disposed to think favourably of this invention, I will send you a model for their inspection.

I am, DEAR SIR,

Your very obedient servant,

EDMUND CARTWRIGHT.

Woburn, Oct. 20, 1802.

This is to certify, that the three-furrow plough invented by Certificate; the Rev. Edmund Cartwright, ploughs a surface of twenty-seven inches each bout, and that on light land a pair of horses regularly ploughs three acres per day with it in a workmanlike manner.

JOHN DUCKITT, *as Bailiff to*
his Grace the Duke of Bedford.

WILLIAM BAXTER, *Assistant.*

June 21st, 1802.

DEAR SIR,

YOU herewith receive the model of my three-furrow Why this plough
saves power. plough.

The saving of hands, and consequently of expence, in a plough of this kind, is obvious; but why there should be a saving of power, may require to be explained.

I need not observe to you, nor to any man who considers the action of a common plough, that a very material part of the labour in ploughing, arises from the friction of *the land side* and *the sole*; of the one against the side of the furrow, of the other against the bottom. In a single plough a certain length and width are required in those parts of it, to make it go steady; and even then the effect would be imperfectly obtained, did not the ploughman assist by the leverage of the handles of the plough. Hence it is clear, that the less disposition any plough has to follow the draught in a strait line, the greater is the labour of working it, because the ploughman in that case is to exert a greater power of leverage to keep it steady. On the contrary, when two, three, or more ploughs are combined, they serve to steady each other, and require comparatively very little power of the lever to keep them in a str. line. Under these circumstances,

Economical
three-furrow
plough.

circumstances, neither the first nor second plough has any sole or land-side whatever; and even the third does not require so much of either as a single plough. I calculate the saving of power from the consideration alone, as equal at least to one plough. What farther power is saved, I attribute to the lightness and compactness of the instrument.

I am willing to think the simplicity of its construction, and the manner of fixing the plough (consisting but of two parts) to the beam, will not escape your observation. When the cutter (for as it is both coulter and share, I can give to it no other single name) requires to be sharpened, or new-laid with steel, by drawing the two bolts the whole is set at liberty.

I make the ploughs to fit each beam indiscriminately; because when the land is too strong, or too foul, to work the three, I take off the second plough, and transfer the third into its place,

You will observe the centre of the whipe-tree shifts. By this contrivance, the power of the horses is equalized, though they may be unequal in strength, the longer lever being given to the weaker horse.

Should the Society wish for any farther information, it will give me pleasure to furnish them with it.

I am, DEAR SIR,

Very truly and sincerely, yours,

EDMUND CATWRIGHT.

Woburn, December 14, 1802.

Charles Taylor, Esq.

Reference to the Engraving of the Rev. EDMUND CATWRIGHT'S Three-Furrow Plough.—Plate II. Fig. 1, 2.

Description of
Mr. Cart-
wright's three-
furrow plough.

Fig. 1. A B, the two wheels of the plough, the wheel B being full one-seventh in diameter larger than the wheel A.

C D E, the three beams of the plough, of which C is the shortest and E the longest: these beams are fixed in the strong cross piece F, at equal distances from each other, and braced by another cross piece from C to E.

G H I, the three cutters which answer the purpose of both coulter and mould-board, each being formed together, or made of one piece of beaten iron. Each cutter is screwed to its beam by the flanging-iron K.

L M

LM, the two handles of the plough, the lower extremities of which are fixed in the two outer beams C E, and connected by a cross piece N, to make them firmer. The handle L is longer than the handle M, in the same proportion as the beam C is shorter than the beam E.

Description of Mr. Cartwright's three-furrow plough.

O P, two upright pieces of iron fixed in the cross piece F, having two holes at their summits for the reins to pass through which guide the horses.

S, an iron bar which slides up and down near one end of the cross piece F, to raise or lower the wheel A.

Fig. 2. Shows a detached portion of the strong cross piece F, to explain the manner in which the whiple-tree shifts (R) are fixed in front of that cross piece, so as to regulate or equalize the power of the horses.

S, a bar of iron, the lower part of which forms the axis of the wheel A, the upper part slides in a groove, in the cross piece, F, and has holes at different distances. It may be retained at any height by an iron pin T, which passes through the cross piece, and one of the holes of the iron bar. The real plough is nine feet long to the extremity of the handles and each cutter turns a nine-inch furrow; from centre to centre of the beams, being nine inches.

VII.

On the State of Science among the earlier Nations of Antiquity: and more especially of those Researches which constitute the Subjects of Alchemy. In a Letter from E. P.

To Mr. NICHOLSON.

Roscommon, April 1, 1804.

SIR,

IT is an authenticated fact, that much of our late scientific acquisition, and many of those facts which the experimental genius of the present age is daily bringing to light as original discoveries, were well known in more ancient periods of the world; and there is abundant reason for supposing that, in chemistry and metallurgy, the philosophers of those ages were superior to those of the present day.

Reasons for thinking that the modern sciences were known to antiquity.

But

But we must mount up much higher than what are called the dark and barbarous ages of modern Europe, or even of any of those revolutions in the East of which history has transmitted any detailed accounts.—Science had *began to decline previous* to the earliest historic relation which is extant, and there appears sufficient evidence that the Greeks and Egyptians, in their hieroglyphics, their allegoric devices, and in their mythologic mysteries, which they had blindly received from their enlightened predecessors, were recording for posterity a series of physics, of which they were ignorant, and which is now gradually unfolding.

**Transmutation
of metals.**

The universal rage for penetrating into the science of alchemy, not only indicates the scarcity and value of gold in all ages, but evinces, I think, that there has always existed some tradition of such a transmutation having been once effected.

**Decomposition
of water.**

The decomposition of water into different gases was certainly once known; and our recovery of that sublime phenomenon, which seems the key to the great laboratory of nature, bids fair to restore to mankind the most important facts which have lain in obscurity for so many centuries.

Alchemy.

Of these, alchemy will probably be one; it has deeply excited the attention of some excellent chemists in this island, with whom I have the honour of being connected: of any progress we may hereafter make, you shall be immediately apprised; and if you, or any of your ingenious correspondents are engaged in a similar course of experiments, we might mutually assist, and abridge each others labours.

I have the honour to be,

Your sincere friend and zealous well-wisher,

E. P.

P. S. A series of experiments on this subject will probably throw considerable light on the lunar (more properly lunatic) tides, the rational phenomena of which you have taken so much laudable pains to elucidate.

Description

VIII.

Description of a Machine for Clearing great Roads from Mud.

Communicated to the Society of Arts, by the Inventor Dr. JOHN WINTERBOTTOM, of Newbury, Berks.*

IN a description of this machine, I shall briefly notice the five principal parts of which it is composed; the frame, the scraper, the chain, the sledge, and the pole; because a very accurate model accompanies this paper, made upon the usual scale of one inch to a foot. Machine for clearing roads from mud.

The *frame* (see *Plate I. Fig. 1.*) consists of two pieces of timber AA, which at one extremity are formed into a pair of shafts BB, and at the other are strongly united by three transverse pieces CDE.

The *scraper* F is placed under this frame-work, in an oblique direction, at an angle of 30° , between two of the transverse pieces CD, and consequently forms an angle of 150° with the line of draught. By this position of the scraper, the machine, when used, actually clears itself from the mud as fast as it is collected, and removes it into a heap on one side, after the manner of a plough.

The *chain* G is connected with a piece of iron-work H, which projects from the lower end of the scraper; for here additional power is required, as the whole body of the mud, which has been collected, must pass off by this extremity. Some advantage has also been gained by making this end of the scraper shorter than the other.

The *sledge* II is constructed upon the upper part of the frame, that by inverting the machine it can be transported without injury to the scraper, over the most rough and stony roads, or pavements, to those places where its use is particularly required.

The *pole* K, which is moveable, serves the purpose of a rudder, that when the machine happens to be forced by any great weight of mud, or solid body of earth, &c. from its proper direction, it can be easily restored to its former position: and it may also be observed, that the moderate pressure

* To whom the silver medal was voted. There is a model in the Society's Repository.

Machine for
clearing roads
from mud.

of the hand upon the pole tends to make the machine steady, and therefore causes it to work to more advantage. In the model, the pole is made only ten inches long, instead of fifteen, that it might occupy less space in the box. The plates in front of the scraper, and upon the sledge, are made of cast-iron.

OPERATION.

For the operation of the machine, two men and four horses are required: one man to drive the horses, and another to take the management of the pole and the direction of the labour to be performed. The horses are to be worked double, as commonly practised, two being employed to draw by the shafts, and two by the chain above described. But the manner of using the machine will be best understood by the following sketch. *Plate I. Fig. 3.*

The first progress of the machine marked No. 1, commencing from the arrow-mark, will remove the mud in a line to the right; the first return, No. 2, will remove another part of it to the left. The second progress, No. 3, will take up what is left by No. 1, besides the quantity which is upon the space now to be passed over, and will remove it all to the right. The second return, No. 4, will operate in a similar manner with regard to No. 2, and remove that to the left. Thus, by four lengths, more than twenty feet wide of a road can be cleared; and this has been frequently performed in the presence of several persons. The number of lengths may be increased at pleasure, according to the width of the road.

In the neighbourhood of London, where there is incessant travelling, it would be advisable to use two machines at the same time, one immediately following the other, as in No. 1 and 3, which will leave a space sufficiently wide for the largest carriage to pass, without disturbing the mud already scraped up.

There is one advantage in the operation of this machine worthy of being noticed, which is, that by the use of it the road is made more even and smooth, the small holes being filled up by the more solid parts of the mud; whereas, when roads are scraped in the usual way, by hand, all the irregularities are increased, and become the future deposits of water; and it is universally known that these puddles, as they are called, are the chief cause of the destruction of roads.

It has been observed, that stones are sometimes forced up by the machine; but it appears to be those only which project in such a degree as to be dangerous to the traveller, and which require to be broken for the more effectual mending of the road.

Machine for
clearing roads
from mud.

I can say nothing concerning the effect of the machine upon dusty roads, having had no opportunity of trying it at that season of the year. When, indeed, the roads are watered, as about London, there is no doubt but a great quantity of that dirt may be removed, which, in a few hours of scorching sun, would again be converted into a body of dust.

If it should be objected, that the machine is too large, and that a smaller one, which might pass over half the space of ground that this does, and might be worked by two horses, would be better; I must beg leave to answer, that, in my opinion, with a less one there would be much labour to little purpose; because this machine, which passes over a space of about six feet and a half, will not, in some places, when the roads are very wet and very deep, leave more than three feet clear, the mud on each side falling in and filling up, to a considerable extent, the space already passed over: it must therefore be obvious, that, under similar circumstances, the track of a smaller one would almost instantly be obliterated.

TESTIMONIALS.

I am so anxious that the Society should have ample satisfaction on this head, that I should be happy if they would, before finally determining on the utility of this machine, condescend to make some inquiries in this part of the country, where it has been publicly tried.

I can however mention, with some pleasure, that several gentlemen, acting as Commissioners of the Roads, have honoured me with their attendance during various experiments; and, having witnessed the very powerful effects of the machine, they have given it their public approbation at the last monthly meeting, when the following entry was made in their minute-book:—

“ At a meeting of Trustees of the London and Bath roads, held at the Globe Inn, Newbury, on Monday, the 21st of February, 1803. At this meeting were present, James Croft, Esq.

Machine for
clearing roads
from mud.

Esq. Frederick Cowslad, Esq. Rev. Thomas Best, Mr. Richard Baily, Mr. Thomas Clark, Mr. John Baily, Mr. Joseph Tanner, Mr. Thomas Pocock.

"Resolved, that the machine invented by Dr. Winterbottom, for scraping off mud from turnpike roads, will be of public utility, and save considerable expence of labour."

After this public testimony in its favour, I might perhaps be excused from producing the certificates of a few individuals: it will, notwithstanding, be necessary to give some estimate of the probable saving to be expected from its use.

In all trials made previous to the 21st of February, the machine had been worked upon no measured extent of ground; but the general effects were such, that several persons of great experience in the management of roads, rated the daily work of one machine only as equal to the labour of fifty or seventy men: fifty being the lowest estimate ever named.

A few days ago I directed some work to be done by measure; and I can now state it as the opinion of two very competent judges, that one machine will clear three miles in a day, twenty feet wide (consisting of four lengths, and making the day's work twelve miles) which is considerably more than 120 men can do in a day.

	<i>l.</i>	<i>s.</i>	<i>d.</i>
120 men, at 2s. per day	12	0	0
Four horses and two men can here be hired to work the ma- chine for the day, at	- 1	5	0
Difference	- - -	10	15 0

At a distance, where carriages run principally in the centre of the road, the chief business in the management of it consists in keeping the sides clear and open. One machine may therefore be occasionally employed in outside work only; that is, may go six miles, and return, (making twelve miles, as just mentioned) with the saving already given.

Whatever surprise these calculations may occasion, the Society will perhaps be satisfied that I have not over-rated them, when I produce the result of a fair experiment, made on the 25th of February, in the presence of four trustees (Frederick Page, Esq. Francis Page, Esq. Mr. Thomas Clark,

Clark, and Mr. John Baily) and others, by which it appears, ^{Machine for clearing roads from mud.} that two miles, by measure, on the road to Reading, were cleared from mud, to the extent of 18 or 20 feet wide, by two machines, in the space of two hours and a half, by the watch; and the work was judged to be equal to the labour of more than eighty men in a day.

The success of this experiment was so satisfactory to the above-named trustees, for I was not present on the occasion, that they directed, without my knowledge, the remainder of our district on this road, extending seven miles, to be cleared in the same manner; and I can now declare, with some degree of pleasure, that this was actually completed by two machines in one day, viz. on the following day, the 26th of February. Of this day's work I have heard it affirmed, by an experienced surveyor, that it could not have been done in one day by 400 men.

I confess that I am myself unable, from the want of practical knowledge on this subject, to form a comparative estimate between the work done by this machine and by hand: I have therefore sought for information from persons of respectable characters, who have been surveyors, or renters of roads for many years: and I have been assured, as well by those who were present at the experiments, as by others who examined the roads afterwards, that it would require sixty men a mile, to do the work in one day, which a single machine will accomplish at four lengths; and it has been already shown, that three miles can, without difficulty, be cleared in a day: one machine will therefore do the work of one hundred and eighty men. But I have taken the average at only two thirds of this estimate, viz. at forty men per mile instead of sixty, being more willing that the power of the machine should at present be under-rated, than that the public should be deceived or disappointed concerning it.

The trustees of the London and Bath roads, being desirous of having these two machines, which had been constructed on my account, and under my own inspection, for making the experiments, I have consented to dispose of them: and as far as I am now able to judge, the price of a machine complete will be about ten guineas.

Finally, I must beg leave to advise those who are inclined to make a trial of this machine, to be careful whom they in-

by it, at pleasure. The person who holds it may direct the Machine for scraper in its proper line; and assist it in overcoming any ob- clearing roads- from mud. stacles it may meet with in its way, or in giving it additional pressure where necessary.

II. Show the two parts of the machine which form the feet, or sledge part of the machine, on which it slides when reversed, and which enable it to be removed from place to place, when the scraper is not in use. These feet are strongly fixed to the timbers AA, and strengthened by a transverse brace betwixt them.

L. Is the iron chain, or back band, which lies upon the cart-saddle of the horse in the shafts, and which supports the shafts.

Fig. 2. Shows, on an enlarged scale, the iron-work, fixed on the outside of the shafts, to which the chain and horse are attached.

Fig. 3. Describes, in a small extent, the track usually made by the scraper in a large way, in four rows, commencing at the arrow mark, in the track No. 1, returning after it has gone any length required by the track No. 2, proceeding again by the track No. 3, and forcing the mud collected by the tracks No. 1 and 3 to the right side of the road, and, on its return by the track No. 4, depositing the mud of the tracks No. 2 and 4 on the left of the road, as is more fully described in the preceding account, and thus clearing from mud a breadth of road twenty feet wide, by four passages of the machine.

IX.

Description and Drawing of an Hydraulic Machine, with an Account of several Inventions of early Date, which have been since brought forward by later Inventors. Extracted from a Foreign Work published early in the last Century. By a Correspondent.

To Mr. NICHOLSON.

SIR,

[HAVE lately met with a work in French, entitled *Recueil* Account of the
Ouvrages curieux de Mathematique et Mecanique, ou Descrip- publication of
on du Cabinet de Mons. Grollier de Serviere, in quarto, *Mons. de Ser-* the Museum of
 printed *vienne.*

Turnery.

printed at Lyons in 1719. The book is divided into three parts, the first of which contains curious engravings of delicate, eccentric, swath and rose-work turnery, the methods of producing which are to be found in Moxon's *Mechanic Exercises*, and in other later works on that subject. Upon this part I shall make no other remark, than by demanding of your correspondents, whether the oval chuck, or engine for turning ovals, was unknown so late as the period above specified. If *Monf. de Serviere* had known it, I should suppose he would have introduced ellipses among the various figures he has exhibited. The second part of the work consists of clocks or

Time-pieces.

time-pieces, more remarkable for some singularity in figure or structure, than any improvement in the art of measuring time; and, the third part contains models of hydraulic machines, with some engines for military and other purposes. Many of the hydraulic machines appear better calculated to be shewn in a model, than carried into effect on a larger scale, and, among the other engines, I see nothing which, at the present day, would much conduce to the entertainment of your readers. I have, therefore, sent you a drawing of the hydraulic engine exhibited in his 49th plate, and after the description, I will mention a few other objects which have been thought of more modern invention.

Hydraulic machines.

Hydraulic machine described. It consists of two pinions working in an elliptical box.

In *Plate IV. Fig. 2.* A and B represent two solid pinions made in wood or metal, and occupying all the interior space of the oval box or chamber CD; in which they turn freely and take into each other. The chamber CD is to be well and solidly made, having an opening below at D, as in the figure, and also at E, where the aperture corresponds with the bore of a pipe F, applied and fixed to the same. Every other part is well closed and secured. *Fig. 3.* represents the cover or cap.

This chamber is properly fixed under the water of the well, or cistern, out of which the supply is to be obtained, and in this situation, the elbow or handle G, *Fig. 1. Plate IV.* is fixed on the square of the axis A. This handle is connected with another H, by the iron sliding piece I, which moves upon the fixed pin K, and obliges its two extremities constantly to move alike. Whenever therefore the handle H is turned by the first mover M L, the other arm G must also revolve together with its pinion, and by consequence the other pinion B.

W hen

When the two pinions turn (so that their upper teeth may constantly approach each other, while the lower recede,) the water which lies between their teeth in the lower part D of the chamber, will be carried round till it arrives at the part C, where it will be compressed by the continual augmentation of water, which is brought thither between the teeth, (or rather by the perpetual diminution of the space between the two uppermost teeth that touch the chamber). So that the fluid will enter the pipe F, and be forced to the intended place.

Thus far I have translated from Monf. de Serviere. The machine appears to be ingenious, and though liable to the objections of wear which you very properly urged against the schemes of O. B. and others *, seems preferable to them in several respects. I think it would be an improvement, to cause the axis A and B to drive by a connection of external wheel-work, instead of depending upon their interior teeth, which require to be well figured and fitted, and kept so, by not loading them with pressure of one surface against the other. It seems scarcely necessary to remark, that we have better methods of connection for work at a distance in our cotton gear and elsewhere, than the sliding piece I; and lastly, I would propose it as a mathematical exercise to determine the law of the velocity of the fluid through F, when the rotation of the machinery is uniform.

Among the machines which I believe have been considered as of later date, but are found in this work, are the engine mentioned in Descaguliers, for raising water by a losing and gaining bucket, and regulated by a fly; the chain pump, for which Cole had a patent, but which is known to have been of very ancient use in the Chinese Empire; the horse mill worked by the wheels of a carriage, as lately proposed by the ingenious Adam Walker; and the gouty chair of Merlin, worked by two small handles connected with a pair of small wheels attached to the fore feet.

Remarks on the engine.

Old engines re-invented.

Losing and gaining bucket.
Chain pump.

Adam Walker's horse mill.

I am, SIR, with esteem,

Your obliged Reader,

R. B.

* Philof. Journal. Quarto series. IV. 468.

X.

An Examination of Dr. Wollaston's Experiment on his Periscopic Spectacles. By Mr. WILLIAM JONES, F. Am. P. S.

To Mr. NICHOLSON,

SIR,

Preliminary observations.

THE inferences that Dr. Wollaston has thought it best to publish in your last month's Journal, instead of a direct reply to my refutation of his new principle of spectacle glasses, are of themselves sufficient to convince any impartial person of the validity of the objections advanced by me in your preceding Journal; and, notwithstanding an extraordinary experiment he has therein related, as made only by himself, I should not have thought it requisite to trouble your readers again, but for the unfounded imputation that he has declared against me, that of having by an experiment deceived myself. I trust, Sir, I may be allowed, in contradiction to this, to observe, that, after more than 20 years experience in the practice of my profession, such as daily administering to decayed vision, and employment in the construction of all kinds of optical instruments, I should not be acquainted with all the various properties of lenses, singly or combined, and especially of so simple and well known a form of lenses as adopted by him, is an idea, that I am confident he will not be able to impress upon the minds of the public. I suggested no new experiment, nor was any one wanting; the definitive laws I adduced, were contained in the works of the best writers on optics, and were sufficient to evince the want of originality and improvement of his meniscus shaped lens. In respect to the experiment by which he attempts to enforce a proof of an advantage in his spectacles, its value will be known by the following account of a repetition of it,

Experiment with a pair of periscopic spectacles compared with a pair of the usual construction.

I am possessed of a pair of his periscopic glasses, mounted in a single steel frame, which cost 10s. 6d. The glasses, I must observe, are different to his proposed form, having of each, the inner side, or that next to the eye so little incurvated, that by any person but an optician, they would be called plano convexes. The focus is four inches, the same as used by Dr. Wollaston in his experiment. In a similar mounting,
with

with double convex glasses of the same diameter and focus, I provided a pair of our own manufacture, and as sold by us at 3s. 6d. These two pair of spectacles were attentively compared together, by myself and several judicious and impartial persons, in the manner as stated by Dr. Wollaston of his. The result was as follows.

The convex glasses being applied as close as possible to the eyes, with the frame attached to the head, the print of a large quarto page was viewed through them, at a distance for distinct vision at their centres, the letters at the distance of about 25 lines, appeared quite distinct or well defined; giving the axis of the eyes a little obliquity to discriminate more lines, an indistinctness or confusion of letters commenced, increasing towards the extremity of sight, and from the lateral aberration of the lenses, the letters were tinged with the prismatic colours. Keeping the head fixed in the same position, the periscopic glasses were substituted. The extent of distinct letters without distortion was nearly as great, but the coloured letters were evidently nearer to the centre, and more numerous than by the other glasses. By inclining the axis of the eyes still more than in the former case, or looking extremely askant through the glasses, a greater extent of lines was observed, but blended with colour and confusion. The optic nerves felt a sensible irritation, evidently from the squinting position of the eyes, a refraction of many superfluous rays, and the consequent increased and unusual magnitude of the images on the retina. The pain in the eyes mentioned by Dr. Wollaston, must have arisen only from this circumstance, and not from the one he represented it to be. By a trial of the old meniscus glass I before mentioned, which is of four inches focus, and corresponds with what he has a patent for, in comparison with one of the above plano convexes, the view of letters was still more extended, but illegible and with much colour, and like the others towards the extremity, of no sort of use for the purposes of vision. Now all this is conformable to the laws of optics, and manifests a property different to that advanced by Dr. Wollaston.

Result. With the double convex lenses. Extent of distinct vision. Indistinct.

With the periscopic glasses, more colour.

Trial of another meniscus.

These several glasses are also at the public service for inspection in our shop in Holborn.

By making the glasses of the above periscopic spectacles nearly planos, Dr. Wollaston's principle is destroyed, and my opinion

Concluding remarks.

opinion evidently verified; that the nearer a meniscus approaches to a plano, the more perfect it will be, as the spherical surface, for the same focus, is diminished, and consequently the aberration besides; admitting that there were any advantage defirable from a great obliquity of the axis of the eyes to those of the miniscus shaped spectacle glasses. I would ask, for what reason has man his head moveable? was it not, that he should place his eyes directly before the object to be viewed, and not subject himself to fallacious ideas of them, by an aukward and revolutionary squinting. From what I have advanced, I doubt not of the public decision, (from a fair comparison of the two kinds of spectacles) in favour of the established double convex spectacle-glasses, for

" Magna est veritas et praevalabit."

I am, SIR,

Your respectful humble Servant,

W. JONES.

Holborn, April 10, 1804.

XI.

Experiments and Observations on the Change which the Air of the Atmosphere undergoes by Respiration, particularly with Regard to the Absorption of Nitrogen. In a Letter from ALEXANDER HENDERSON, M. D.

To Mr. NICHOLSON.

SIR,

Whether nitrogen be absorbed in respiration.

I TAKE the liberty of communicating to you a brief detail of some experiments on respiration, which were undertaken chiefly with a view to determine the absorption or non-absorption of *nitrogen*, a point which has been hitherto much controverted, but which I flatter myself to have now sufficiently ascertained.

The gasometer.

These experiments were performed by means of a gasometer, capable of containing about 2200 c. inches, and graduated so as to shew a difference of 2 c. inches. In breathing from this apparatus, the inconveniences from friction were very inconsiderable. Towards the end of the experiment, however,

over, when the air in the gasometer became vitiated, and the respirations fuller and quicker, some disagreeable effects were experienced from the smallness of the breathing tube, and from it becoming, in some measure, choaked by the vapor from the lungs, which was condensed in it.

After closing the nostrils, and making a forced exhaustion of the air in the lungs, a full inspiration was made of the ^{Method of re-}atmospherical air in the gasometer, and this inspiration as well ^{spiring.} as the subsequent expiration, was measured, by means of the graduated scale. The air of the gasometer was then respired as long as possible, and till the oppression about the chest became so great as to oblige the experimenter to desist, and before closing the stopcock, and separating the tube from the mouth, the magnitude of the last full inspiration and expiration was accurately observed.

In all our experiments, the bulk of the atmospherical air ^{and general} was considerably diminished by respiration, the quantity that ^{effect, as noted;} disappeared varying from five to eight c. inches per minute. This diminution, however, is partly to be attributed to the condensation of the oxygen gas, when converted into carbonic acid gas, in which form its bulk is diminished about one fourth. In order to obviate any objections that might apparently arise from the difference of the inspirations and expirations at the commencement, and at the conclusion of the experiments, they were, as we have already mentioned, carefully noted; and if any difference did occur, it was deducted on the side where the preponderance took place.

In proceeding to examine the chemical qualities of the air, ^{Eudiometrical} considerable difficulties were at first experienced for want of ^{examination.} an accurate eudiometer. Several trials were made with *nitrous gas*, which affords the most easy and expeditious mode of analysing atmospherical air. But, independent of the ^{Nitrous gas} inaccuracies which may arise from the number of vessels it is ^{exceptionable.} necessary to have recourse to in employing it, this test is liable to still greater objections, from differences in its degree of purity, from its absorbing a portion of nitrogen, and from its combining with a greater or less proportion of oxygen, according to the diameter of the vessels employed, the length of time which it is allowed to remain in contact with atmospherical air, and the degree of agitation used in mixing them.

The

Seguin's eudiometer with phosphorus objected to ;

The eudiometer with *phosphorus*, as recommended by Seguin, is by no means free from these disadvantages. It is subject to the same inconveniences as the nitrous gas, with respect to the frequent change of vessels; and, if we may credit the observations of V. Humboldt, is liable to several sources of fallacy from the different proportions of oxygen, which the phosphorus absorbs, and from its combining also with nitrogen. A portion of carbonic acid may also be generated, if the phosphorus be not perfectly free from impurities. The eudiometrical test, which has been lately recommended by Davy, viz.

and also the impregnated sulphate of Davy.

a solution of the *pale sulphate of iron*, saturated with nitrous gas, is not altogether exempt from the inconveniences which attend the use of the latter substance, and has the additional objection, that a portion of nitrogen is generally disengaged from the solution, after the absorption of oxygen is completed.

Sulphuret of alkali or lime absorb oxygen very slowly ;

The *sulphurets of alkalis and lime* have hitherto been regarded as among the most accurate tests of the purity of atmospherical air. They, however, absorb oxygen but slowly, and several days may elapse, before the absorption be completed. It is therefore, in general, necessary to have recourse to complicated and uncertain calculations, in order to adjust the result of the experiment to the variations of the surrounding atmosphere. To remedy this objection to their use, the application of heat was proposed by Guyton, by which means a speedy absorption was effected; but, besides, that some difference of the product may result from the method employed, the apparatus itself is objectionable, from its size and inequality of dimensions.

and are not completely amended in this respect by heat.

The apparatus of Dr. Hope affords great advantage.

Luckily for the progress of eudiometrical science, these obstacles to the attainment of an accurate knowledge of the constituents of the atmosphere, seem to be now, in a great measure, removed, by the invention of an apparatus by Dr. Hope, which unites the advantages of neatness and simplicity with those of extreme accuracy and expedition, and of which you have already given a very full description in your Journal*. In the trials which I have made with this instrument, the substance found to answer best as a test of the atmospherical air was the *sulphuret of lime*, which is more

Sulphuret of lime was used.

* Vol. VI. pages 61. 210.

readily decomposed than the sulphuret of potash, and is consequently better adapted for the purpose of expedition. In most of the experiments, the absorption was finished in about twenty minutes, if sufficient agitation had been employed. The only objection, to which the above test may seem liable, is, that, according to some, it absorbs a small proportion of the nitrogen, as well as the oxygen of the atmosphere. This opinion, however, does not appear to be well founded, for in several of our experiments, where the quantity of oxygen absorbed appeared to be unusually small, and where the agitation was continued for a much longer time than necessary, no perceptible alteration in the bulk of the air was observed *.

It does not appear to absorb nitrogen.

The method of proceeding in the analysis of the air, was briefly as follows: after having ascertained the purity of the atmospherical air by means of the eudiometer above-mentioned, and knowing the exact bulk of the air contained in the gasometer, the total quantities of oxygen and hydrogen in it were calculated by a very simple process. This air was then respired, and its diminution marked as has been already described. After respiration, a portion of it was introduced into the eudiometer, and its carbonic acid was absorbed by means of lime water, (for which the above-mentioned instrument was found extremely convenient). Freed from the carbonic acid, the air was now subjected to the action of the sulphuret of lime, and the relative quantity of nitrogen contained in it was thus discovered. Then, by deducting the quantity of carbonic acid, and of oxygen gas, contained in the air of respiration, from the total quantity that remained after respiration, we procured the proportion of nitrogen, which abstracted from the total quantity before respiration, gave the proportion of nitrogen absorbed. Thus, let a represent the original quantity of nitrogen; b the carbonic acid, and c the oxygen of the air of respiration, and M the bulk of the residual air; $M - b + c = n$, or residual nitrogen, and $a - n = x$ or nitrogen absorbed.

The process. Previous examination of the air,

and after respiration.

Experiment I. June 16, 1803.

600 c. inches of atmospherical air were respired, for four minutes, at the temperature of 63° .

Exp. 1, 2, 3.
The bulk of nitrogen absorbed in respiration was about 1-40th of the whole, or

* De Marti is of opinion that the hydrogenated sulphurets absorb nitrogen only when recently formed. Vide Journal de Physique. Tom. LIII. p. 176.

Before

about 1-32d of
the nitrogen.

Before respiration, 100 parts contained of

Oxygen - - - - .22

Nitrogen - - - - .78

After respiration, quantity diminished to 570 c. inches,
100 parts found to contain of

Carbonic acid - - - - .07

Oxygen - - - - .14

Nitrogen - - - - .79

$570 - 39.7 + 80 = 450.3$.

$468 - 450.3 = 17.7$ c. inches or quantity of nitrogen
absorbed.

Experiment II. June 18, 1803.

600 c. inches atmospherical air were respired, for four
minutes, at the temperature of 64° .

Before respiration, 100 parts contained of

Oxygen - - - - .22

Nitrogen - - - - .78

After respiration, quantity diminished to 570 c. inches,
100 parts found to consist of

Carbonic acid - - - - .08

Oxygen - - - - .12

Nitrogen - - - - .80

Quantity of nitrogen absorbed, therefore, = 12 c. inches.

Experiment III. February 11, 1804.

1000 c. inches atmospherical air were respired for the
space of $4\frac{1}{2}$ minutes, at the temperature of 57° . Barometer
= 28.78.

Before respiration, 100 parts contained

Oxygen - - - - .22

Nitrogen - - - - .78

After respiration, quantity diminished to 962 c. inches.

After respiration, 100 parts contained

Carbonic acid - - - - $.07\frac{1}{2}$

Oxygen - - - - .13

Nitrogen - - - - $.79\frac{1}{2}$

Quantity of nitrogen absorbed, therefore, = 15.1 c. inches.

Comparative re-
marks on these
experiments and
those of Davy.

These different experiments, (which I have selected from
among many others) agree in the general result, viz. that a
portion of nitrogen is abstracted from the atmospherical air,
by the blood in its passage through the lungs, although the
amount

amount is somewhat less than has been stated by Davy, who makes it equal to about 5 c. inches per minute. Yet I am inclined to think, that this is more a difference in appearance than reality; for if we consider that the most of Davy's experiments give the result of the changes produced on the air by a single inspiration, or by a small number of respirations, while in the experiments just described, a large portion of air was breathed for a considerable length of time, so as to become, at last, unfit for the due performance of respiration; it is probable that the blood could no longer produce the same alterations in its properties, that took place when a purer atmosphere was inspired. It is also natural to suppose, that the quantity of air consumed in respiration varies in different persons, and in the same person at different times. An approximation to the truth, therefore, is all that we can expect to obtain in the determination of this question; and we must rest satisfied with the knowledge of the important fact, that nitrogen is absorbed by the human body in respiration.

The striking uniformity in the analysis of the atmospherical air, by means of the sulphuret of lime, occurred also, as I have with pleasure observed, to Dr. Thompson in the numerous trials which he has made on this subject; and furnishes a most interesting problem for the investigation of the chemist and natural philosopher.

I am, with much Respect,

SIR,

Your most obedient Servant,

AL. HENDERSON, M. D.

Edinburgh,

April 12, 1804.

XII.

Observations and Experiments tending to ascertain the Causes of those Irregularities in Chronometers, which are generated during considerable Intervals of Time, and have been ascribed to external Causes. By Mr. JOHN HALEY, Jun.

To Mr. NICHOLSON.

SIR,

HAVING seen in the last number of your very useful Journal some remarks on chronometry, I have thought the accompanying observations might not be unacceptable. If you should consider them worthy of insertion, I shall take the liberty of communicating the results of some experiments I am now making in the same line.

I am, SIR,

Your most obedient and humble Servant,

JOHN HALEY.

March 29,
1801.

25, Cleveland Street,
Fitzroy Square.

Latent causes of error in time-pieces.

If the errors of the machine be ascribed to foreign causes, improvements will cease.

The foreign causes are of little effect: For many machines have performed wonderfully well, while subject to their action.

MY design in writing the following pages is to develop certain latent causes of error, and their mode of operation, which have been found in a greater or less degree to affect every machine hitherto constructed for measuring time.

It is certainly the part of a watch-maker to search for and remove every mechanical cause that may affect the going of his machines, before he hastily concludes that the causes of those errors which are generated in a length of time, (and which have escaped his notice,) are foreign to its structure; such as the influence of the oil, the varying different density of the atmosphere in which the wheels move, &c. For such opinions when once adopted, must greatly tend to slacken his pursuits and put a stop to his hope of farther improvement.

That these foreign causes do in fact produce errors deserving much notice in the best adjusted time-keepers is probably an erroneous notion. For if so, how could so many of different constructions have been found to perform for very long periods of time, exposed to all these causes, with a wonderful degree of

of accuracy. Indeed Mr. Cumming, in his treatise on clock and watch work, has supposed that the influence of the oil may be in some cases even beneficial to the performance of the machine; though it must be allowed he there speaks of such as have no provision against the effects of heat and cold. And though Mr. Mudge in some of his letters to his Excellency the Count de Bruhl does write, that when the air was moist in Devonshire (where he made and tried his machines) they retarded their rates; yet the quantity of error produced by this supposed cause was so small, that it gave him no uneasiness with regard to their fate. But the very different performance of machines on the same construction, strongly induce me to believe that most of the errors hitherto found in the best time-keepers have been produced by latent mechanical causes coexistent with the machines, or rather, with the times they were first put in motion.

Various opinions have been entertained by gentlemen of scientific and mechanical acquirements, and by artists upon this subject. One in particular has very generally prevailed, namely, that the errors have arisen from inequality of power derived from the main spring, and the train of wheels. That very great errors will be produced by these causes, (if not removed by good workmanship,) must on all hands be admitted; but where the execution has been correct, the errors will be trifling indeed, and must always remain nearly the same. The late Mr. Arnold, on being asked by a Committee of the House of Commons his opinion of the Remontoire, said, that it was only a help to bad workmanship.

Mr. Harrison's opinion of the Remontoire was different from that of Mr. Arnold; for he expected to arrive at perfection in his machine from introducing it, though by his method of application, he did not detach his escapement from the whole of the train of wheels. The invention was undoubtedly a great proof of his superior ingenuity, and merited high praise. However, on finding the going of his watch not to answer his expectation, he attributed its irregularity to the thermometer not having its due effect; and asserted, "that if it could properly be put into the balance, the watch would go within a few seconds a year," which assertion has since been proved erroneous.

Mr.

Mudge's Remontoire for winding up every vibration.

He supposed it would detect unknown causes of error.

Mr. Mudge's comprehensive mind suggested a Remontoire that should be wound up at every vibration of the balance, and he attached it to a 'scapement which was certainly very superior to Mr. Harrison's, and for uniformity of excellence in its going, is more to be depended upon, in my opinion, than any other which has hitherto been applied to a portable machine. Mr. Mudge in the construction of this 'scapement, had almost rendered his Remontoire useless with respect to one purpose for which he introduced it. His son, in a book entitled a *Narration of Facts*, (pages 46 and 47, in the margin) has published an extract from a manuscript of his father's, wherein he writes as follows, "was I to make a watch myself upon this reasoning, I should not expect it to be by any means perfect, but I cannot help thinking, that I should in the first essay get rid of so many, and so great errors, that the cause of those that remain would be more comeatable than when blended as they are now, &c." I say, that he had, in the formation of his admirable 'scapement, nearly annihilated those errors, the cause of which he proposed to arrive at the knowledge of by the introduction of the Remontoire; it is much to be lamented that this great mechanic's powers failed him through age and infirmities, at the time when he had almost arrived at the knowledge of those causes of error which he so earnestly sought after. And we may presume from the superiority of his penetration, that he would also have accomplished the means of removing them, and consequently of giving to his machines all the perfection of which their principles were susceptible.

Mudge's watches accelerated their rate, at first.

Dr. Maskelyne's account of them.

It is observable that Mr. Mudge's watches, (which I consider from their construction as having the causes of error which I shall endeavour to develop, operating in them in a less degree than any other time-keepers,) in almost every instance accelerated their rates of going, while under trial at the Royal Observatory. The Rev. Doctor Maskelyne has, I think, done mechanics an essential service, by publishing his very judicious and accurate remarks upon their going while under his care. He observes, among other facts, "that the watches did generally accelerate their rates of going with sometimes retarding them a little, that they accelerated their rates less in the second trial than in the first, and least of all in the last trial, and that towards the latter part of that trial the

the watch *green* retarded its rate." If we carry our observations farther, we find, that, in their subsequent trial which took place at Mr. Dutton's, (the watches having had nothing done to them,) they both retarded their rates throughout. I will endeavour to shew why these watches did accelerate and retard their rates in this manner.

The errors of opposite tendency that did exist in these machines were so small, and I may add, were such a length of time in generating, that it became very difficult for Mr. Mudge to ascertain their cause. Where was he to look for them? It was not to the 'scapement only his search was to be confined, but he had also to traverse through the whole train of wheels: For though he had applied his Remontoires beyond the train, yet the last wheel (if there was any inequality in the power derived from the main spring or train) would press sometimes stronger, and at others weaker upon the pallets of the Remontoires; which pallets the balance in its vibrations (by means of the pins in the crank affixed to it) had alternately to unlock, before it could be impelled by the unbending of the Remontoire springs—therefore, as he supposed the causes were not determined to the 'scapement, he knew not where to fix them *.

Great difficulty of ascertaining the causes of error in Mudge's time-pieces, if in the train.

It was an opinion (in writing) of Mr. Mudge's, "that the simple principles of all watches are the same and perfect, the errors found in them are therefore not errors arising from the principles, but from imperfections, inseparable from all mechanic operations."

Opinions of mechanics. 1. Of Mudge, that time-pieces err not from principle, but imperfect execution.

The late Mr. Arnold asserted before a Committee of the House of Commons, that, though he put no oil to his 'scapement, yet it wore less than any other part that was subject to wear.

2. Arnold: that his 'scapement wore less than other parts.

Mr. Emery told the same Committee, that the 'scapement which he made for his Excellency the Count de Bruhl, after a model of Mr. Mudge's, was very difficult to execute, but when made, it would not easily wear out. It is a known fact, that this watch went with an unusual degree of excellence.

3. Emery: that Mudge's 'scapement would not easily wear.

* For drawings and descriptions of Mr. Mudge's 'scapement, the detached 'scapement now in general use, and several others, see Philos. Journal, quarto series II. p. 49.

4. General observation; that when the 'scapement wears there is no more regularity.

Inference: that the errors in the best time-pieces are thus caused.

Distinct statement: that the alterations of rate are caused by wear in the 'scapement.

Short description of the usual detached scape-ment. 1. Scape wheel, 2. (face of) detent, 3. passing spring, 4. unlocking pallet, 5. impulse pallet.

Consequences of wear. 1. If the wheel (point) should wear, it will sooner escape and strike the pallet.

And still more it is the observation of every watch-maker, when speaking of the verge and horizontal 'scapements, that when once the verge or cylinder begins to wear, there is an end of all good performance, and that the going of the watch becomes from that period totally incorrect.

These several testimonies tend to induce a belief, that the errors which still remain in the best time-keepers, are produced chiefly by this cause; and this cause must exist from the moment the machine is first put in motion, although the errors will not manifest themselves to any considerable extent, (particularly if the materials are of the best kind,) until the machine has been going for some time.

I therefore submit it as a fundamental principle, that the principal cause of the errors found in the best adjusted time-keepers, consist in the wearing of the different parts of the 'scapement; that so long as by the act of wearing the relative proportions of its parts are preserved, that errors of contrary kinds compensate each other in the general action, the machine will go correctly; but so soon as those ratios of the parts are altered by wearing, the watch will go either too fast or too slow.

The scape wheel of what is termed the detached escape-ment, say of the late Mr. Arnold, or any other of them in use, (for their principles are the same, and differ only in modification,) is looked upon a jewel in the detent. The detent has a small spring fastened to it, which reaches considerably beyond the locking jewel. Some watch-makers have given the name of passing spring to it, because, the little pallet in one vibration of the balance passes it without disturbing the detent, and in the next vibration, the same pallet by laying hold of its point, lifts the detent out of the wheel, which immediately impels the balance by means of the great or impulse pallet*.

To simplify our deductions, let it be supposed that the point of the passing spring does not wear at all, and I think it will appear to every person acquainted with the 'scapement, that if the points of the teeth of the wheel should wear, the watch must go faster. For the wheel must then escape the locking jewel of the detent sooner, and will necessarily impel the balance sooner also; consequently each impulse given to

* Philos. Journal. Quarto II. 54.

the balance must be beaten quicker and quicker, as the points of the teeth of the 'scape wheel wear more and more.

But on the contrary, let us now suppose that the points of the teeth do not wear, and the passing spring wears; in this case, as the balance in its vibration will have a greater space or portion of a circle to move, before it can arrive at the spring, and unlock the wheel, in order to receive the impulse, every such impulse will be made later and later, and the watch must necessarily go slower and slower as the point of that spring wears away more and more. There is another part in this 'scapement, namely, the striking face of the tooth, in which, if wear takes place, the tendency I presume will be to lose, in consequence of the wheel acquiring a greater drop, or having farther to go before it can overtake and strike the pallet. This part of the tooth I here supposed to wear, and which first comes into contact with the pallet, is not the point, but a little nearer the centre of the wheel; and as the wheel follows the pallet, it must be some portion of time longer before it can arrive to begin the impulse. In the above example I have supposed the detent and both pallets to be jewelled: if they were not, the errors would be greater.

If the passing spring wear, the unlocking, and consequently the impulse will be later.

If the face of the teeth should wear, the drop will be more and the impulse later.

Now we find that, according to the above reasoning, although there are two parts of the 'scapement in which, if wear takes place, the tendency will be to lose; leaving out of the question the oil becoming glutinous, the springs losing their elastic force, &c. and only one part, in which if wearing takes place the tendency will be to gain, yet, we find in most machines the tendency to gain is generally predominant. I will show why I think the teeth of the wheel wear more than either of the other two parts: there is not only friction, but percussion also takes place in the three different parts of the 'scapement before-mentioned; the little roller or pallet that lifts the detent out of the wheel strikes against the point of the passing spring on coming into contact in order to unlock the wheel; in the next place the wheel strikes against the pallet when it begins the impulse, and lastly, the wheel again strikes against the jewel of the detent when it locks upon it: I will not attempt to determine which of the two former percussions is the greatest, or which will occasion the greatest retardation of rate, but will say, that the last is greater than either of the other two by much, and in general we find that the wheel is locked upon

Thus there are two kinds of wear which tend to produce loss, and one to produce gain.

—but the gain predominates,

—and this because the stroke of the wheel in locking is much that greater, that

those of the
unlocking and
the impulse.

that part of the face of the tooth nearest the point; now as repeated strokes will wear as well as friction, I take it that the wheel wears more from this cause (particularly where the execution has been favourable for it) than either of the other two parts, and therefore most time-keepers made upon this construction have a greater tendency to gain than they have to lose.

The advantage
of clocks or ma-
chines with short
vibrations de-
duced from the
smaller per-
cussion.

If it should be admitted that percussion will wear in any material degree, I submit that it will lead to the discovering why clocks whose pendulums make the shortest vibrations go better than others, and also the cause of the great disparity there is between the going of the common verge watch and that of table and long eight-day clocks; in both clocks and watches constructed with two pallets upon the recoiling principle, each pallet, with the accumulated force it has obtained by the vibration of the balance or pendulum, meets the wheel which is coming a contrary way when it comes into contact, and a percussion takes place; therefore the longer the vibration of the pendulum or balance, the greater must this percussion be, and consequently the wear also: hence may be supposed one reason why spring or table clocks that make shorter vibrations go better than verge watches, and long clocks, which make shorter vibrations still, go better than either. I readily admit there is one, and perhaps many weighty objections to this reasoning, viz. that some clocks upon the recoiling principle are found to go as well or even better than others on the principle of the dead beat, though the execution in each has been of equal excellence. For an answer to this objection, I refer the reader to Mr. Cumming's elaborate treatise upon clock making; who in some part of the work, asserts, the reason to be, that the plane of action of the pallet does not sufficiently subtend the angle of vibration; by which means the power applied is administered too suddenly in the dead beat; to which I add that the percussion is also greater in this case in the dead beat than in the recoil, although the pallet does not meet the wheel in opposition, and the wearing of the teeth of the wheel may be increased by that means. There will be among many others one very serious objection to my supposition that the wearing of the wheel can produce error in a long pendulum clock, viz. the power of the pendulum being so much superior to the motive force, it cannot be affected by it; to which I shall only observe that the results in practice are sometimes very different from theoretical conclusions.

Some objections
considered.

The

The mode of operation by which friction is said to accelerate the rate of machines, is thus pointed out by the trade in general, viz. that whenever it takes place by the action of the wheel upon the pallet or pallets, (in watches or clocks constructed with one or two pallets), or in the verge holes by the action of pivots therein, it impedes the motion of the balance or pendulum, shortens its vibrations, and in consequence the machine goes faster: how does this agree with Mr. Harrison's opinion? which was, that large arcs are naturally performed in less time than small ones. Mr. Cumming, in the work of his before alluded to, has intimated, that nothing conclusive can be drawn from different arcs, because they are effects and not causes of error.

In the Repertory of Arts and Manufactures, No. 33, in the year 1796 or 7, there is a description of a Remontoire 'scape-ment, which is completely detached from the train of wheels. The errors of this machine must be determined to the 'scape-ment alone, because no errors in the going can possibly arise in any other part of the watch. For if any additional power, however great, be applied to the train, the arcs of the balance will not be in the least affected by it: in the opinion of all or most who understood its principles, its going must be more perfect than that of any machine before invented.

In the following remarks upon that 'scapement, I shall call, what is in the before-mentioned description of it termed a snail pallet, a locking pallet. In the commonly termed detached 'scapement, every tooth in the wheel is locked in succession upon the jewel of the detent, and the effect arising from wearing of the teeth does not very soon shew itself, there being 12, 15 or more (at pleasure) in number, but in this remontoire 'scapement before every impulse there is only this individual locking pallet to supply the place of a wheel, (if I may so express it) the wear therefore must be 12 or 15 times as great, and its effect must manifest itself very soon.

In the years 98 and 9, I executed four or five of these 'scapements, and tried the going of each. They all went in the same manner, viz. for the first twenty-four or thirty hours the machine would vary nothing; the day following it would gain one or two seconds, the next day seven or eight, and on the fourth or fifth day it would gain 30 or 40. I observed, the more the machine gained, the larger were the arcs of the balance;

Whether gain be occasioned by shortened vibrations.

An escapement completely detached.

In this the wear on the locking is greater because the action is on a single face.

Account of the construction and early performance of several of these.

lance; if the machine was set a-going without oil being applied to the locking pallet, it would not go many days before it would stop, and if set in motion the balance would perform only three or four vibrations, when it would come to rest again: in this state, if oil was applied to the locking pallet, it would set a-going more lively than ever, the balance making considerably larger arcs, and the machine gaining upon its former rate in a very surprizing manner.

Explanation of the irregularities;—from wear or change in the parts, and not from friction only.

Should it here be enquired, how were these effects produced? I think it would be answering in a vague and indefinite manner to say merely,—that it was occasioned by friction; for the mode by which it operated is certainly necessary to be pointed out: I suppose that by friction the point of the locking pallet (which was made of steel) wore shorter, by which means it locked shallower upon the jewel in the detent, therefore the balance unlocking the Remontoire sooner, the impulse to the verge was given quicker as the point of the locking pallet became shorter by wear; also, the shallower the locking pallet locked upon the jewel of the detent, the less was the resistance to the balance in unlocking it, and the drop upon the impulse pallet became less, therefore the arcs of the balance became larger in proportion as the locking pallet wore shorter: I can never believe that the above effects, viz. acceleration of rate and large arcs were produced by friction abstractedly; for had it been so, they would have ceased the moment oil was applied to the locking pallet, instead of which the effects became more apparent.

Remedy. An intermediate wheel was introduced, to render the locking faces more numerous.

It was not till after various examinations of the 'scapement alluded to, and making two or three others of the same kind, that I formed the foregoing conclusions respecting the cause of the error that had puzzled me; when I had fixed upon the cause in my own mind, I determined upon a remedy, which was, by applying an intermediate wheel between the remontoire and the balance, instead of the locking pallet; I contrived to fix a short weak jewelled detent upon the remontoire axis, which moved round with it and locked upon the intermediate wheel, which had twenty-four teeth; as the locking pallet had before locked upon the detent, I placed also a detent upon the other side of the intermediate wheel, to lock it, and by a particular contrivance made the intermediate wheel to shift one tooth at every vibration of the balance; after which
alteration,

alteration, the former effects totally ceased; in order to make the intermediate wheel (which was loose) shift one tooth at every vibration, I made the detent which locked it strong, and inserted a long jewel into it, having an inclined plane that filled the space between each two teeth that always kept the wheel in a certain position, the consequence of applying the strong detent was, that the point of the passing spring (or in other words the unlocking part) wore very fast, and occasioned a very considerable retardation of rate, which continued without intermission until the watch stopped from the point of the passing spring wearing so much, that the balance could not unlock it. I must observe, that the arcs of the balance were in this case not perceptibly altered, and the retardation not nearly so great as the acceleration in the former. I afterwards, by another contrivance, which it would be difficult to explain, remedied the evil attendant upon a strong detent, and produced an escapement perfectly to my wishes, that had no propensity either to accelerate or retard its rate.

It succeeded.—
Strong detent to
shift the wheel

—produced wear
in the passing
spring;

—and retarda-
tion.

Remedy.

It is clear to me, that friction or wearing will, in some cases, make a machine accelerate, and in others retard its rate of going, therefore I presume if we could hit upon some method of preserving the ratios of the detached 'scapement permanent, by giving the different parts a durability, they have not hitherto possessed, we might produce machines of this simple construction that should retain an equal rate of going for long periods of time, and not look to chance for their success; if the actions of the 'scapement were unalterable (which I imagine might be made by jewelling) we should then have only the influence of the oil to oppose our success, which by becoming glutinous, will, I think occasion, if any change, a retardation of rate, yet if it be good, the effect, I suppose, will not be very great in a moderate length of time.

The simple de-
tached 'scape-
ment would per-
form well for
long periods of
time if preserved
from wear.

Having been on the coast of Coromandel and Ceylon, and other parts of the East Indies, for a series of eight years, I had there an opportunity of trying an oil to watches, which artists here are unacquainted with; being in camp in the Myfore country, in the years 1791 and 2, for a period of about eighteen months, I could scarcely procure a drop of olive oil that was not rancid; I therefore substituted in room of it what is there called cocoa nut oil, which retains its fluidity to the last. I never knew an instance of its becoming glutinous, nor

Cocoa nut oil
recommended
for time pieces.

does

does it evaporate soon; I have found it in jewelled, verge, and horizontal watches, in that warm climate, after a period of three years, in a very fluid state, and in sufficient quantity. That oil may be easily procured here by breaking the shell of the cocoa nut, drying the nut in a very gentle manner, and afterwards expressing the oil from it, which is yielded in a great quantity; it assumes a concrete form in a very moderate degree of cold, but how far that may be a good objection against its use in this climate remains to be determined.

Mudge's first watch stopped for want of oil on the pallets.

I will conclude these observations, after a few remarks upon the stopping of Mr. Mudge's first watch, while under trial at the Royal Observatory; Mr. Mudge found, after a good deal of pains taken in investigating the cause, that it was for want of oil being put to the pallets (or locking part) of his 'scapement: I think this case so parallel to that of Mr. Haley's machines stopping from the same cause, that it tends in some measure to prove the truth of what I have advanced: Mr. Mudge had thought when he finished this machine, that it was altogether unnecessary to apply oil to this part, as he mentions in a letter to his excellency the Count de Bruhl, upon the subject of its stopping; and in the same letter he writes to the following effect, that when he had applied oil to the pallets, the watch went as well as before, or, as it did when first set a-going; that the arcs of vibration had fallen off but a few degrees, yet the watch went ten seconds a day faster than the last-mentioned rate, and from any thing that suggested itself to him, he could not see why it should not have gone rather slower than faster through length of time. I will venture to suppose the following to have been the cause of the machine going faster, and of the balance measuring smaller arcs in consequence.

Effect when oil was supplied.

Explanation
The teeth of the wheel wore shorter; escaped sooner, and (q.)

The points of the teeth of his 'scape wheel had become shorter * by constant wear, however small, and escaped the pallets (or rather hooks of the pallets, as they have been termed) sooner; therefore the opposite pallets which impelled the crank gave the impulse sooner to the balance, and in my mind, the watch may be supposed to have accelerated its rate from this cause; on the other hand, by the wearing of the teeth, the wheel did not wind up the remontoire springs quite so

* Refer to the figure, Philos. Journ. 4to series, Vol. II. p. 49.

high,

high, therefore there arose in some degree an impediment to the balance, which might occasion the arcs to be shortened, because, before the wheel was set at liberty, or unlocked from one pallet, in order to wind up the other remontoire spring, the balance (by means of the crank affixed to it) had alternately to wind up each remontoire spring a little higher, and in short, so much higher than the wheel had before wound it, as the momentum of the balance was equal to; therefore, as I said before, when the teeth of the wheel became shorter by wear, they would not wind up the remontoire springs quite so high; in which case the crank would engage the pallets on the opposite side of the remontoire axis a little sooner, by which means the balance would receive a check in its vibrations, and the arcs would certainly fall off. These, which I have mentioned, are the only reasons which have occurred to me to shew why the remontoire machine I made should have its arcs of vibration increased when the watch went faster, and, on the contrary, Mr. Mudge's should have its arcs shortened when it went the same way.*

—and the remontoire springs would be less wound up; whence the balance would have more to do alone.

As to Mr. Mudge's watches, *green* and *blue* retarding their rates while under trial at Mr. Dutton's, after having gone through their public trials at the Royal Observatory, it should be remembered they had not been cleaned, and that the oil being thicker, must have had a share in producing that effect, and perhaps a principal one; for as to friction in the impulse or unlocking part, there was none, it was totally annihilated, by the verge and remontoire axis moving on the same centre; if any wear could take place there, it must be by percussion and pressure only: likewise it may be considered, that the teeth of the wheel (or locking part) having been blunted by wear with former going, would have larger surfaces to resist the wear and its accelerating effect during this trial; besides, it will be readily believed by those acquainted with Mr. Mudge's scape-ment, that the wearing of his wheel and its pernicious effect, cannot be so great as in that of the detached 'scapement.

The retardation of Mudge's other time pieces ascribed to insipidation of the oil.

* It appears to me, that if the impelling pallet came to a stop before the instant of unlocking, no other than the last effect, in Mudge's machine, would take place; except that the resistance to unlocking might become less. Would the crank and levers wear and diminish the effect of the acting pallet? — N.

A Memoir

XIII.

A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake and other American Serpents. By BENJAMIN SMITH BARTON, M. D. From the American Transactions, Vol. IV.

(Continued from p. 285 of Vol. VII.)

Experiment of
fascination by a
snake.

THE facts which came under the notice of Mr. Vosmaër, at the Hague, are curious, and deserve to be mentioned. But they do not appear to me to be proofs of the existence of an infectious or mephitic vapour proceeding from the mouth of the rattle-snake. I am not at all surpris'd that the birds and mice that were put into the cage, along with this reptile, should exhibit the motions which were observed by the Dutch naturalist. When the little animals squatted down in a corner of the cage, they were, most probably, impelled by the instinct of fear, which is so powerful, and so extensive, in the vast family of animals. When they run towards the serpent, it may have been fear that actuated them.

Perhaps partially
observed.

In conducting a series of experiments, it is ever a matter of importance, that the mind of the experimentalist should be free from the dominion of prejudice and system. Perhaps, facts are never related in all their unadulterated purity except by those, who, intent upon the discovery of truth, keep system at a distance, regardless of its claims. The strong democracy of facts should exert its wholesome sway. I cannot help thinking, that if Mr. Vosmaër had disbelieved the fascinating faculty of serpents, the conclusions which he would have drawn from his experiments, just mentioned, would have been somewhat different. But of this I cannot be certain, and, therefore, I shall not avail myself of the supposition.

Other facts
which contradict
the notion,

Some experiments which have been made in this city, do not accord with those of Mr. Vosmaër. The birds, which were put into the cage that contained the rattle-snake, flew or ran from the reptile, as though they were sensible of the danger to which they were exposed. The snake made many attempts to catch the birds, but could seldom succeed. When a dead bird was thrown into the cage, the snake devoured it immediately. He soon caught and devoured a living mole, an animal much more sluggish than the bird. A few days since,

since, I had an opportunity of observing the following circumstance. A small bird, our snow-bird*, had been put into a cage containing a large rattle-snake. The little animal had been thus imprisoned for several hours, when I first saw it. It exhibited no signs of fear, but hopped about from the floor of the cage to its roof, and frequently flew and sat upon the snake's back. Its chirp was no ways tremulous; but perfectly natural: it ate the seeds which were put into the cage, and by its whole actions, I think, most evidently demonstrated, that its situation was not uneasy.

I do not relate this latter fact with any intention to disprove the notion, that the rattle-snake possesses the faculty of charming. For the observation was made on the seventeenth of last month, which is somewhat earlier than the time when our snakes usually come out of their dens. The snake, too, which was the subject of the experiment, appeared to be very languid, and had not eaten any thing for a considerable time. We ought not, therefore, to suppose him possessed of the fascinating faculty at this period; since, I presume, that this faculty, did it exist at all, is subservient to the purpose of procuring the reptile its food. The fact is, perhaps, valuable in another point of view. It seems to show, it does show, that the mephitic vapour proceeding from the rattle-snake, allowing that such a vapour really existed, was, in no respect, injurious to the bird.

If the mephitic vapour of the rattle-snake were productive of the effects attributed to it by Mr. de la Cépède, and other writers; and, especially, if this vapour extended its influence to animals situated at a considerable distance from the reptile, the atmosphere of the rattle-snake would often be a kind of *Avernus*, which many animals would avoid, and which would generally occasion the sickness or death of those that were so unfortunate as to come within its sphere. But how different is the case! The abodes of the rattle-snake are the favourite haunts of frogs, and many species of birds, which often pass the seasons of their amours and generation in clouds of mephitism; uninjured, and undestroyed. How often has the rattle-snake been known to continue, for days, at the bottom of a tree, or even a small bush, upon the branches of which

No mephitic vapour was emitted by the snake in the experiment,

nor in the wood at large.

* The *Emberiza hyemalis* of Linnæus.

the thrush or cat-bird are rearing their young ! This would be a suitable situation for the mephitic vapour to exert its noxious influence ; but, in our woods, such influence has never been perceived.

Other instances
in proof.

Birds of the eagle and the hawk kind have been seen to soar, for a considerable time, above the spot occupied by a rattle-snake, and at length to dart upon the reptile, and carry it to their young. Neither the parent-bird nor its young ones, have ever been known to receive any injury from the snake's vapour. Possibly it may be said, this vapour was dissipated, or greatly diluted, in passing through the air.

Whether other
animals emit
mephitic va-
pour.

A mephitic, or fetid, vapour emanates from the bodies of many animals, besides the rattle-snake ; from the opossum *, and the pole-cat †, for instance. The vapour of these quadrupeds would be as likely to affect birds, &c. with asphyxy, as that of the rattle-snake. And possibly it does. There is, certainly, one thing in favour of the supposition. The opossum, in particular, is noted for his cunning in catching birds.

I shall conclude this part of my memoir by observing, that the odour of the rattle-snake is said to be agreeable to some persons.

Question, if the
agitated animals
may not have
been already
bitten.

Mr. de la Cépède's second mode of explanation is much more plausible. I have already observed ‡, that it was the system of Sir Hans Sloane, who affected to ground it upon experiments. It is adopted by the author of the well-written account of de la Cépède's *Natural History of Serpents*, in the *Monthly Review* §.

Mr. de la Cépède presumes that, " for the most part, when a bird, a squirrel, &c. has been seen precipitating itself from the top of a tree, into the jaws of a rattle-snake, it had been already bitten ; and that its whole conduct, such as its crying, its agitation, its leaping from branch to branch, &c. are all effects induced by the violent operation of the poison, thrown into its body, by the reptile.

* Didelphis Opossum.

† Viverra Putorius.

‡ See pages 30 & 34, note.

§ Appendix to the second volume of the *Monthly Review* enlarged. p. 511.

An attention to facts constrains me to reject this attempt to- Most probably
wards a solution of the question, which I am considering. I not;
shall arrange my chiefest objections under two heads.

First. We are pretty well acquainted with the most prominent effects produced by the poison of the rattle-snake, in various species of animals. It must be admitted, that there is a considerable variety in these effects, and a great difference in the strength of these effects. In one animal, the poison produces an high degree of inflammatory action in the system; in another, the most striking primary effect is a somnolency, or drowsiness. In one animal, the poison does not produce any obvious effect upon the system for many minutes; in another the effects are almost instantaneous *. But in almost every instance in which the poison of the rattle-snake has been successfully thrown into the body of an animal, there ensue a set of symptoms, very different from the actions of birds and squirrels when under the supposed fascinating influence of the serpent-kind. It is not necessary to detail, in this place, these various symptoms, because I have already done it in a paper, which is printed in the third volume of the *Transactions* of our Society †, and because these symptoms cannot be unknown to the members of the Society. It will be sufficient to observe, that two of the most universal effects of the poison of the rattle-snake, I mean the extreme debility and the giddiness, which commonly almost immediately succeed the bite, will preclude the possibility of a squirrel's, or a bird's, dancing from branch to branch, flying about, and running to and from the serpent, for a considerable time, before it becomes a prey to its enemy. Besides, the force of fascination is often kept up for a much longer term of time than any small animals are known to live after a successful bite by the rattle-snake. But, perhaps, it may be said, that the rattle-snake, like some of our wasps, knows how to inject into the animal, which he means to devour, any given quantity of his subtle poison. Here, the analogy will not apply: but I have not time to point out the various instances in which its failure is conspicuous. —because the effects are more speedy and very different from what are related of fascination.

* A small dog that was bitten in the side by a large rattle-snake, reeled about, and expired, seemingly suffocated, in two minutes. This was in the month of August.

† No. xi. p. 110 and 111.

The system of fascination implies that the animal may be relieved and escape.

Kalm mentions a well-known fact, which will be admitted to have considerable weight in destroying the force of this part of Mr. de la Cépède's system. "The squirrel being upon the point of running into the snake's mouth, the spectators have not been able to let it come to that pitch, but killed the snake, and as soon as it had got a mortal blow, the squirrel or bird destined for destruction, flew away, and left off their moanful note, as if they had broke loose from a net. Some say, that if they only touched the snake, so as to draw off its attention from the squirrel; it went off quickly, not stopping till it had got to a great distance." "Why," continues our author, "do the squirrels or birds go away so suddenly and why no sooner? If they had been poisoned or bitten by the snake before, so as not to be able to get from the tree, and to be forced to approach the snake always more and more, they could however not get new strength by the snake being killed or diverted *."

(To be continued.)

SCIENTIFIC NEWS.

Very extensive Table of Squares.

ABOUT three years ago, a large quantity of mathematical papers were brought to Oxford, which had belonged to Mr. Councner, an attorney at Bloxam, in the north of Oxfordshire. They consisted chiefly of mere transcripts and collections from different publications: But there were two works among them, which must be excepted from this description, and which prove Mr. C. to have been a man of singular industry.

The one was a table of sines and tangents for every second of the quadrant. It is probable, that this table was completed before the year 1760; but it would have been of no value, even if it had not been superseded by Mr. Taylor's publication. For Mr. C. seems, in this instance, to have been ignorant of the true method of calculating, and to have

* Travels into North-America, &c. vol. ii. p. 209 & 210. It will be easy to discover what part of Kalm's reasoning, in the above quotation I admit.

only

only interpolated some old tables, without allowing for the variation of the differences. The other work is really valuable, it is a table of squares and cubes to a much greater extent than that, which was published by the Board of Longitude in 1781, under the care of Dr. Hutton*. This table was calculated for the squares of all numbers from 1 to 128540, and for the cubes of all from 1 to 26560. But there is an interval in the squares from 28261 to 29061. It appears to have been originally left in the manuscript, although it is impossible to conjecture the reason of the omission. The grocer, who had purchased the papers, had begun to tear this table before it could be rescued out of his hands; but as he had not got beyond the number 6000, this circumstance is comparatively of little consequence. In all other respects the work is complete. It is written in a clear hand, and (having been examined in several hundred places) appears to possess a degree of correctness, which may in general be depended upon.

Among the papers were proposals for printing this table; they are dated 1758. There were, likewise, two sheets of an introduction to them, printed in 1761. At this distance of time, it is impossible to ascertain the reason, which prevented the publication; but it is not improbable that Mr. C. might have been stopped by the great expence of the undertaking. Had Dr. Hutton been in possession of this manuscript, it might have saved much of his valuable time, and if any man of science should be employed in continuing his work, it will be useful to him to know that Mr. Cuncer's has been preserved.

P. S. Mr. Cuncer had, likewise, calculated all the powers from the 1st to the 13th (inclusive) of the series of numbers from 1 to 20. And he has continued this, for the nine digits, as far as the 26th power. *S. R.*

* * I received the preceding notice from the respectable gentleman whose initials are subjoined; and am enabled to give such farther information as may conduce to render the papers of public utility. *W. N.*

* Dr. Hutton's table is for the squares of all numbers from 1 to 25000, and the cubes of all from 1 to 10000.

Phosphoric

On Phosphoric Rings; from a Correspondent: R. B.

Phosphoric
rings.

I DO not know that any one has explained the phenomenon of those dense cloudy rings which are produced when phosphorated hydrogen arises through water, and explodes in the air; an effect similar to what is sometimes seen on the firing of cannon. Upon close inspection the fact appears to take place thus. The bubble of hydrogen, containing phosphorus in solution, rises and takes fire. Phosphoric acid is separated in the form of white smoke; through which dense mists the expansive action is directed upwards. A fluid opaque ring, denser than the atmosphere is thus formed, the inner surface of which is made to ascend by the rapid stroke, while the external part has received little or no impulse. The natural consequence is a quick rotation of the ring, from within outwards, which shews itself to the sight, and seems, in some manner or another, as if it kept the parts together.

How to measure the Contents of any Pipe by a very ready Method.

Contents of any
pipe in pounds
or gallons.

MY ingenious correspondent, Mr. Woolf, sometime ago gave me a ready method of measuring the contents of a pipe, as follows:

Square the diameter in inches, and the product will be the number of pounds of water in every yard length of the pipe. Or if the last figure be cut off or considered as a decimal, the remaining figures will give the ale gallons in the yard.

D. Waterbottom's Machine for Sawing, Drawn from Model.



Fig. 1.

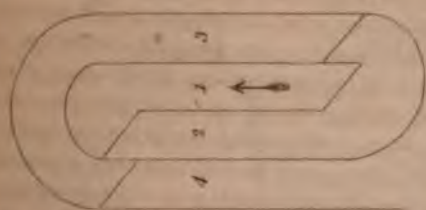


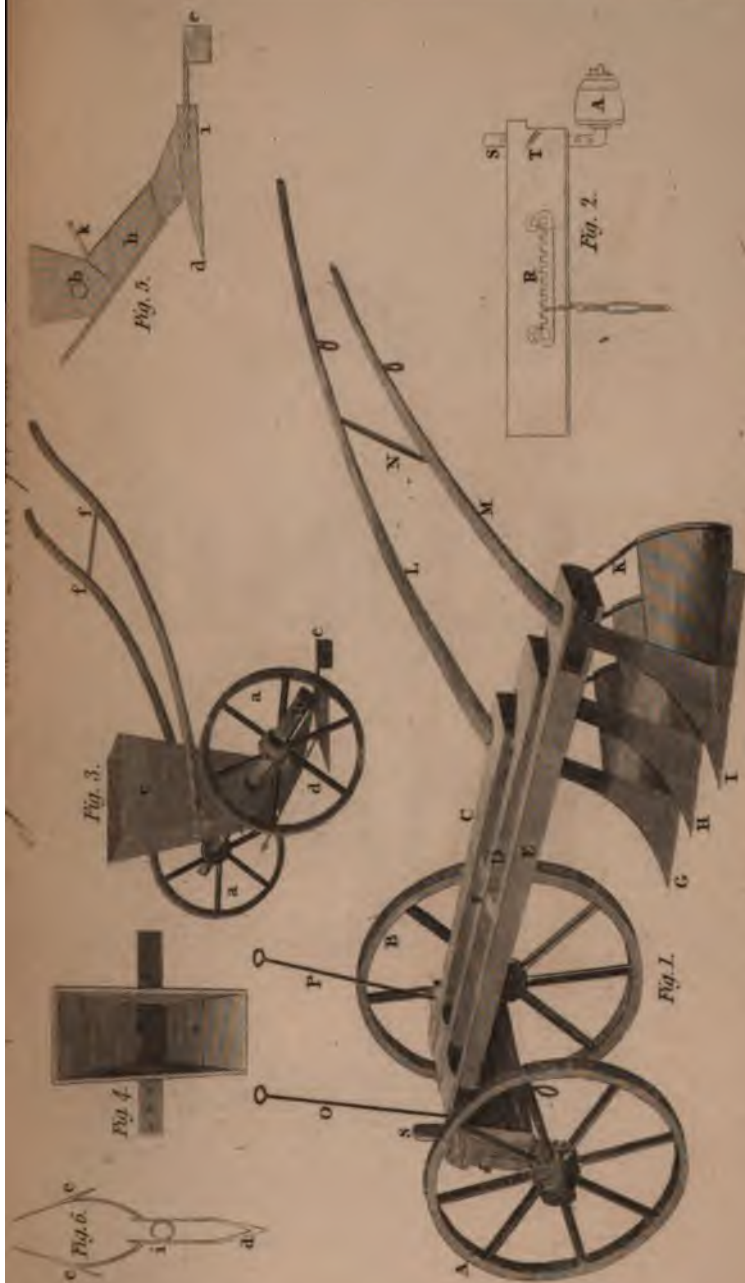
Fig. 3.



Fig. 2.

Scale of Feet.





The Rev. Co. Cartwright's three furrow Plough.

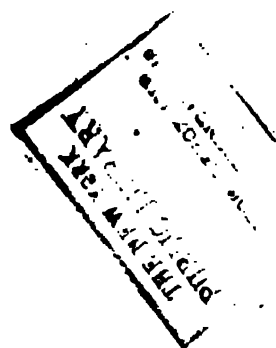




Fig. 1.

*Figure of
the Earth.*



Fig. 2.

Galvanic Apparatus.



Fig. 3.

New Galvanic Trough. W. N.

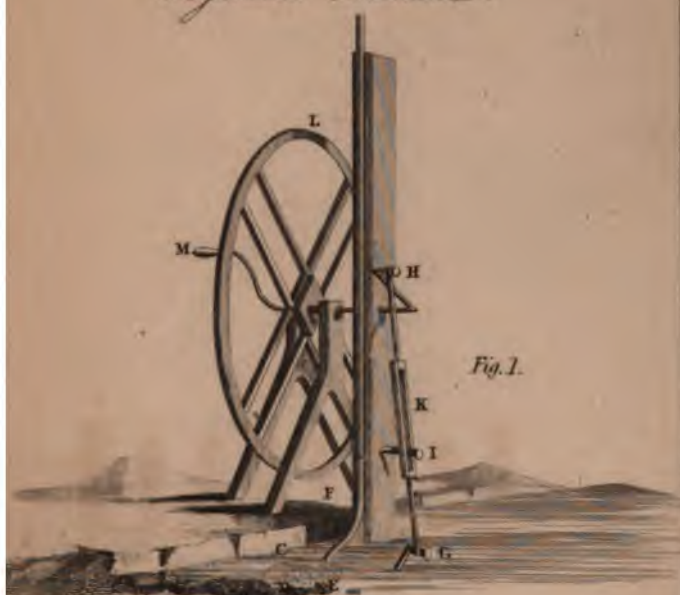


Fig. 4.

Perspective view of Fig. 3.



Hydraulic Machines.



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AND
THE ARTS.

JUNE, 1804.

ARTICLE I.

Letter from Mr. EZEKIEL WALKER, on the Methods of observing the Longitude at Sea ; with an Exhibition of the very great Accuracy of the mean Result from a Number of Chronometers.

To Mr. NICHOLSON.

SIR,

THE two most approved methods of finding the longitude ^{Longitude found} at sea, are those by lunar observations and by time-keepers. ^{by lunar observations and by} Although those methods are now brought to considerable per- ^{time-pieces.} fection, yet they can only be looked upon as in a progressive state towards that point of precision, at which the astronomer and the artist have long been labouring to arrive.

The lunar theory is still but imperfectly understood; but per- ^{Both methods} haps the time is at no great distance, when it shall be as well ^{still improvable.} known as the theory of any other planet. Nor is it unphilosophical to suppose, that some property of matter, which is at present unknown, may hereafter be discovered, by means of which a time-keeper may be so constructed as to perform as well at sea as a regulator on shore.

These are conjectures, though probable, that I shall not insist upon any further, but proceed to inquire into the merits of the two methods in their present state of perfection.

The lunar method will be for ever subject to those intervals wherein the moon cannot be seen.

The imperfection of the lunar method is, that in consequence of bad weather, or the moon's not being above the horizon, it sometimes happens that the necessary observations cannot be taken for many weeks. And these long intervals between one lunar observation and another, must for ever happen from the same causes; and those causes are such as no human art can remove.

Instances of the much greater frequency of observations by time-pieces.

La Perouse, in his voyage round the world, was at one time 31 days without taking a single lunar observation; but he had, within that time, 20 observations by his time-keeper. At another time during the same voyage, 24 days elapsed without a lunar observation; but he had 20 observations by his time-keeper. Lord Hugh Seymour, in his voyage to the West Indies in the year 1796, had been at sea near six weeks before the first lunar observation was obtained, in which period the longitude had been obtained 30 times by his time-keepers.

Time-keepers may stop; or may alter; or be neglected in winding up; or may suffer from accident.

What has just been mentioned sets time-keepers in a very favourable light, but it remains to inquire into their imperfections. 1. A time-keeper may stop. 2. It may alter in its rate of going. 3. It may not be wound up through forgetfulness. And, 4. It is liable to other accidents, even in the most careful hands. But these are defects which may be removed, and a result obtained, which will be more perfect than by any time-keeper that has yet been made.

Method of obtaining a more perfect result from time-keepers; proposed upwards of 20 years ago.

The method which I have to mention must be known to many gentlemen, as I had the honour to lay it before the commissioners of longitude in the year 1783. The method, however, seems either very little known, or not much attended to. La Perouse made no use of it in his voyage round the world; hence it may be supposed that it was unknown in France at that time. Nor is it once mentioned by Mr. Wales in his excellent treatise on the method of finding the longitude at sea by time-keepers; although he had "been pretty intimate with the subject for near forty years"*.

It consists in taking the mean result of a number of machines.

The method which I have recommended depends upon a particular use of time-keepers as they are now constructed. Although the best time-keeper be too imperfect a machine to be depended upon for determining the longitude in long

* Preface to Wales on the longitude, p. 5.

voyages, yet if five, six, or more, be taken to sea in the same ship, the longitude computed by each separately, and the mean of their results taken, it will come exceedingly near the truth, even at the end of three or four months.

Another very great advantage resulting from this method is, ^{This method saves the time in case of neglect in winding up,} should one of the time-keepers stop, it may be set going again by the rest; or should one of them be found to go very incorrectly, its rate may be rejected, and the longitude still determined by those which shall be supposed to go well; in short, this method is almost intirely free from all those accidents to which a single time-keeper is liable.

The two following examples, which are taken from the rates ^{and affords a wonderful degree of precision. Examples.} of chronometers that have been determined in fixed observatories, will show the perfection of this method more clearly.

In the first example, which contains a period of 30 days, there are only two instances in which this method differs so much as a single second from mean time. In the second example, containing a period of six weeks, the greatest error on any one day, during that time, is only $5''.9$, and the total error at the end of that period, is only $1''.9$.

It may be said that these are favourable specimens, but sup- ^{If the chronometers had performed ten times worse, the error would have been less than one minute per month.} posing that these chronometers had gone *ten times worse* than they did, the greatest error in the first example would have amounted to no more than $11''.3$; and the greatest error in the second example would not have exceeded a single minute.

FIRST EXAMPLE.

Table of the going of three chronometers, and their mean result.

Day	Mr. Arnold's Chronometers.			Mean Daily Rate of Three Chronometers.	Total Error of Three Chronometers.
	Daily Rate of No. 36.	Daily Rate of No. 51.	Daily Rate of No. 59.		
	Feb. 1779	Feb. 1782	Feb. 1783		
1	+ 0".79	- 0".3	- 1".5	- 0".33	- 0".33
2	- 0 .15	+ 1 .1	1 .5	0 .18	0 .51
3	+ 0 .15	- 0 .4	+ 0 .3	+ 0 .02	0 .49
4	0 .00	+ 0 .5	0 .7	0 .40	0 .09
5	- 1 .16	0 .0	0 .0	- 0 .39	0 .48
6	+ 1 .34	- 0 .7	- 1 .6	0 .32	0 .80
7	0 .81	1 .3	0 .4	0 .30	1 .10
8	- 0 .26	0 .0	+ 1 .4	+ 0 .38	0 .72
9	+ 0 .56	0 .5	0 .2	0 .09	0 .63
10	0 .07	0 .0	0 .6	0 .22	0 .41
11	- 0 .45	+ 0 .2	0 .0	- 0 .08	0 .49
12	0 .39	0 .3	- 1 .2	0 .43	0 .92
13	0 .25	0 .6	0 .4	0 .02	0 .94
14	0 .35	0 .9	+ 1 .4	+ 0 .65	0 .29
15	1 .48	- 0 .9	1 .2	- 0 .39	0 .68
16	+ 0 .09	0 .2	0 .2	+ 0 .03	0 .65
17	0 .30	0 .5	2 .2	0 .67	+ 0 .02
18	- 0 .58	0 .7	1 .4	0 .01	+ 0 .06
19	0 .77	0 .1	1 .6	0 .24	0 .30
20	0 .90	0 .9	- 0 .6	- 0 .80	- 0 .50
21	0 .00	0 .0	+ 0 .2	+ 0 .07	0 .43
22	1 .55	0 .0	1 .8	0 .08	0 .35
23	0 .48	+ 1 .0	0 .6	0 .37	+ 0 .02
24	0 .76	0 .7	1 .6	0 .51	0 .53
25	0 .29	0 .9	1 .2	0 .60	1 .13
26	0 .68	1 .0	- 1 .2	- 0 .29	0 .84
27	1 .74	1 .1	0 .8	0 .48	0 .36
28	0 .49	0 .1	+ 0 .5	+ 0 .04	0 .40
Mar. 1	1 .86	0 .4	0 .7	- 0 .25	0 .15
2	- 0 .80	- 0 .7	+ 1 .8	+ 0 .10	+ 0 .23

SECOND

SECOND EXAMPLE.

	Mr. Mudge's Chronometers.		Mr. Arnold's No. 86.	Mean Daily Rate of Three.	Total Error of Three Chronome- ters.
	Green.	Blue.	Daily Rate.		
	Daily Rate.	Daily Rate.			
1	- 1".12	- 0".27	- 0".3	- 0".56	- 0".56
2	.61	+ .06	0 .1	.22	0 .78
3	.97	- .40	+ 0 .1	.42	1 .20
4	.64	.40	0 .3	.24	1 .44
5	.16	.16	0 .1	.07	1 .51
6	.11	.05	—	.05	1 .56
7	.64	.41	- 0 .8	.62	2 .18
8	.47	.57	0 .7	.58	2 .76
9	.35	.13	1 .1	.53	3 .29
10	.99	.45	0 .7	.71	4 .00
11	.05	.30	0 .1	.15	4 .15
12	.74	.37	+ 0 .1	.33	4 .48
13	.36	.12	0 .7	.07	4 .41
14	+ .04	+ .08	0 .2	.11	4 .30
15	+ .05	- .38	0 .5	.06	4 .24
16	- .26	.34	- 0 .6	- .40	4 .64
17	.00	.45	1 .2	.55	5 .19
18	+ .43	.18	0 .8	.18	5 .37
19	- .36	.49	0 .6	.43	5 .85
20	+ .07	.19	0 .0	.04	5 .89
21	+ .39	.23	0 .0	+ .05	5 .84
22	.44	.17	+ 0 .2	.16	5 .68
23	.59	.04	+ 0 .0	.18	5 .50
24	.49	.46	- 0 .2	- .06	5 .56
25	.65	.46	0 .3	.04	5 .60
26	.20	.74	+ 0 .1	.14	5 .74
27	.86	+ .08	0 .3	+ .41	5 .33
28	.53	- .41	1 .5	.54	4 .79
29	.74	.85	1 .3	.40	4 .39
30	+ .67	.07	0 .2	.27	4 .12
31	1 .50	+ .85	0 .6	.98	3 .14
1	.80	- .03	1 .8	.86	2 .28
2	.87	.23	1 .0	.55	1 .73
3	.63	+ .02	2 .6	1 .08	0 .65
4	1 .28	.16	0 .4	.61	0 .04
5	.43	- .51	2 .2	.71	+ 0 .67
6	.84	.42	0 .8	.41	1 .08
7	.61	.18	0 .6	.34	1 .42
8	.25	.68	0 .2	- .08	1 .34
9	.38	.54	- 0 .6	.25	1 .09
10	.41	.39	0 .2	.06	1 .03
11	- .83	- .27	+ 2 .2	+ .92	+ 1 .95

Table of three
other chronome-
ters.

From

Hence the longitude at sea may be well determined,

From these examples we may see the nature of fortuitous events, how regularity rises out of irregularity; the error of one time-keeper correcting that of another in a wonderful manner; and as the number of time-keepers increases, the error will decrease, until it be almost annihilated. Consequently the method of finding the longitude at sea is no longer doubtful, as it may, by this method, be found to any degree of precision that may be useful in navigation.

and the expence is inconsiderable.

The only objection that can be advanced against it is the expence; but the sum of two or three hundred pounds bears no proportion to the value of a British Squadron, nor even to the value of a single East India ship; and this sum would purchase as many time-keepers, at their present reduced prices, as would, I presume, be sufficient to secure any ship from that danger which might arise from the want of knowing the longitude.

EZEKIEL WALKER.

Lynn, April 19, 1804.

II.

On Galvanism. By C. WILKINSON, Esq.

May 6, 1804.

To Mr. NICHOLSON.

SIR,

Introductory letter.

BEING now engaged in some philosophical lectures at Bath, I have had frequent opportunities of conversing with Dr. Gibbes, a gentleman of considerable scientific information, relative to his opinions as to the apparent decomposition of water by the galvanic process. As many gentlemen of eminence in this department of philosophy are converts to his doctrine, I beg leave, through the medium of your valuable Journal, to state the outlines of Dr. Gibbes's theory to the public, with a few cursory observations which have occurred to me.

I am, Sir,

Your's with great respect,

C. WILKINSON.

No. 19, Soho-Square.

Soon

Soon after the publication of the valuable discovery of the decomposition of water by the galvanic process, by Messrs. Nicholson and Carlisle, Richter, an eminent German philosopher, observing that water becomes decomposed by wires placed at a considerable distance from each other, could not conceive that the same identical particle of water could be influenced by two wires at the same instant of time; and hence he conjectured that the Lavoisierian theory relative to water being a compound substance must be incorrect.

In the decomposition of water by galvanism the effects are produced at great distances asunder.

Dr. Gibbs supposes water to be an elementary principle, and that the two gases which are produced are compounds. Thus the oxygen gas which is disengaged from the zinc end of the battery, is taken to be a compound of water and positive electricity; the water constituting its ponderable part, while the hydrogen gas given out by the other wire connected with the copper end of the battery is concluded to have water also for its ponderable part in a state of combination with negative electricity.

Dr. Gibbs supposes the oxygen to be water and plus electricity, and the hydrogen to be water and minus electricity.

Dr. G. considers positive and negative electricity as two distinct principles, which when in a state of union constitute caloric. Thus in the explosion of a mixture of oxygen and hydrogen gases, the two electricities enter into union and form caloric, while the respective ponderable bases are precipitated in the state of water.

That plus and minus electricity compose caloric.

Upon this hypothesis, the circumstances of the evolution of flame and heat, together with the production of water, are capable of explanation.

Hence flame and heat,

Also upon this supposition some notion may be formed, why the gases are produced in the vicinities of each wire, and why oxygen gas must be evolved from the wire connected with the zinc end of the battery, and hydrogen gas from the wire united with the copper end.

and the galvanic phenomena.

This ingenious supposition, which does great credit to the well-known abilities of Dr. Gibbs, in the first point of view seems to receive additional support from its apparent simplicity. But upon more minute investigation, I am induced to think it will be found more complicated than the Lavoisierian theory, and by no means so general in its application.

Examination of the theory.

In the first place it should be proved that there are two such distinct principles as positive and negative electricity; and in support of this, the experiments of *Eeles, Symner, Atwood, &c.* are by no means conclusive.

The two electric principles not proved.

When

Nor the decomposition of ammonia explained.

When pure ammonia is exposed to the galvanic action, we have, as before, hydrogen from one wire, and nitrogen from the other. How can these productions be explained upon the principle of Dr. Gibbs?

This theory supposes oxidation to be the addition of water and subtraction of electricity. But the pile requires oxygen from the atmosphere.

This able physician further observes that oxydation is a combination of water with the metallic body, consonant to the opinions of Priestley, Watt, and others.

When a galvanic pile is placed under a receiver situated over water, while the galvanic process goes on, by having a metallic circuit from the top to the bottom plate, a loss of air ensues, and which is found to be the pure part of atmospheric air: if the pile be placed in an exhausted receiver, the galvanic process is very trivial, and the plates very slightly oxydated. How upon the principles of Dr. Gibbs can we explain these phenomena? If oxydation be merely the combination of water with the metal, why should the pure part of atmospheric air be thus separated while the metals are in immediate contact with so much water?

Oxydation appears to require the disengagement of electricity.

Whether galvanism be the cause or the effect of the chemical change induced in the metallic substance, they appear to me to be contemporaneous results, similar to what appears in the immersion of iron in a solution of the sulphate of copper, and whether the iron be dissolved prior to the copious precipitation, no experiments I am aware of can decide. From various circumstances it appears that oxydation cannot take place unless the combined electricity of the metal be capable of being disengaged. This is rendered evident in a very happy experiment of Dr. Ash, who having been the associate of the much-lamented Humboldt* in his scientific pursuits, may now be deemed more acquainted with the minutiae of galvanism than any other person. This gentleman has remarked that when a plate of zinc is immersed in a weak solution of sulphuric acid and water, a decomposition takes place, the oxygen base combines with the metal, while the hydrogen is disengaged in a gaseous state. When a plate of silver is immersed, no decomposition takes place; the very instant a contact is effected between the two metals, whether by portions out of the fluid or in the fluid, then the silver is immediately acted on and disengages hydrogen also, and itself becomes oxydated.

Dr. Ash's experiment.

* Humboldt died lately at Acapulco, of the yellow fever, while attempting to perfect his geological observations.

In

In the first instance the evolution of electricity appears to be *Explanation.* the result of a chemical action of the sulphuric mixture on the zinc, while in the latter, no action is induced on the silver till some alteration is effected as to its electrical state; as we well know from other galvanic experiments, that of zinc and silver, when thus combined, the zinc evinces positive signs, and silver negative; it is hence evident that an abstraction of electricity from silver is requisite prior to its capability of being acted on by the sulphuric acid.

If I might presume to submit to some of your chemical *Additional re-* readers, an examination of such compound of silver as may *marks.* thus be effected; a sulphate of silver is actually formed, it might lead to some important discoveries. I am every day more and more persuaded that light and electricity are the two most active agents we possess, and a more minute enquiry into their respective energies, I have no doubt, would enable us to explain many physical changes in the material world, with which we are at present perfectly unacquainted.

The very intense light disengaged from charcoal by means *Intense light* of my extensive apparatus, has occasioned me to be frequently *from galvanic* honoured with the attendance of Dr. Herschell; from some *combustion.* conversation I have had with him, I am in hopes he will be induced to institute some experiments relative to the evolution of light, so nearly approaching to solar light. If these should be conducted, I shall do myself the honour of stating them to the public through the medium of your valuable Journal.

III.

On the Formation of Snow. By G. A.

To Mr. NICHOLSON.

SIR,

THE remarks contained in the enclosed pages, may not, perhaps, evince any originality of thought, nor more than would occur to the most superficial observer; yet the suggestions of some persons, frequently lead to the observation of valuable facts by others. Therefore, Sir, you will make whatever application of the paper you consider best.

If

If you think it worthy of insertion in your valuable Journal, it will be considered as an honorary obligation conferred on,

Your's, most respectfully,

23d April, 1804.

G. A.

On the
formation of
snow;—

The frequent changes of the weather that have taken place during the last winter, having induced me to direct my attention to meteorology; I confess, that the manner in which philosophers account for some of the phenomena that occur, is not, to me, altogether satisfactory.

—supposed to be
effected by
electricity.

It is not surprizing, that electricity (with the immediate agency of which we are so little acquainted) should be resorted to, as the grand agent in all meteorological phenomena. Accordingly we find, that snow, and indeed every variety of weather we experience, is considered to be more or less effected by the electric fluid.

Questions re-
specting the
mode, &c.

Snow is generally supposed to be the vapours of the atmosphere, disengaged by the electric fluid, and frozen.

But it appears to me, that before we receive so vague an explanation, the following questions might be asked:—

What are the vapours of the atmosphere composed of?

By what laws, and in what manner does the electric fluid act, either in the formation of snow, or as a component part of it?

Electricity sup-
posed not essen-
tial to snow;

I shall now offer a few remarks to strengthen a supposition, that the electric fluid is not engaged in, or in the least essential to the production or existence of snow.

—but a change
of wind.

By an attentive observation of all the circumstances that have attended the fall of snow, during the last winter, I have, in almost every instance, found that it is accompanied with, or rather preceded by a change of the wind; and that the wind, previous to the fall of snow, blew from some point between the *South*, and the *West*; and afterward from some point between the *East*, and the *North-West*.*

* If it is observed, that we sometimes have snow, without the wind changing to any of the points above-mentioned, or, even *without a visible change* to us; yet it does not militate against the following remarks; for it has been observed by aeronauts, that different strata of air blow from *opposite points* at the same time.—Therefore, notwithstanding a south wind may prevail at the surface of the earth, a superior stratum may blow from the North.

Such

Such being the facts, is it not probable, that a *change of the wind is the cause of snow*?

Now let us examine, whether such a cause will produce such an effect.

The winds that blow from any of the points between the *South* and the *West*, by coming from warm climates, and passing over, perhaps, a very large tract of water, where there is a powerful evaporation going on, must possess a very great degree of humidity, and are most commonly, of a temperature between 45° and 60° of Fahrenheit.

Warm winds
meeting cold
deposit their
vapours in snow.

The winds which blow from any of the points between the *East* and the *North-West*, by coming mostly from such high latitudes, and passing over immense fields of ice, where evaporation is undoubtedly greatly impeded, cannot be supposed to contain much water in solution, but must bring with them very great degrees of cold.

Namely, north-
easterly meeting
south-westerly
winds.

Now let us suppose that a north wind of any temperature between 32° and 0° (which it generally is, in superior strata of the atmosphere) meets a south-west wind, as before-mentioned, the consequence will be, that the intense cold which accompanies the former will convert the water with which the latter is impregnated into ice; and the instantaneous application of cold is probably the reason why snow is produced in what we call *flakes*; for before the vapour can concentrate itself into large particles, or drops, it is arrested by the intense cold.

In this view, the formation of snow appears to be a beautiful chemical phenomenon; for the warmer air, having a greater affinity for the colder air than it has for the water which is held in solution, the water is disengaged, crystallized by the cold, and precipitated in the form of snow.

Formation of
snow.

It is generally observed, that it is unusually cold for half an hour or an hour before the fall of snow, and warmer afterwards. Might not this be accounted for, by considering that the adverse wind must meet with considerable resistance, in effecting either a *union* with, or a *passage through* a stratum of air surcharged with water, and consequently must be in a great degree reflected back again, not in the perpendicular, but as radii from a center, in an oblique direction, part of which must descend to the earth. And it will undoubtedly be warmer, after

after

Formation of
snow.

after the stratum of north wind has either forced a passage through or effected a union with the south-west wind.*

Though I have not, in the preceding observations, considered the electric fluid as at all essential to the production of snow, yet I do not deny the presence of it. That snow contains the electric fluid, cannot be doubted; but it does not follow, that the latter is *necessary* to the existence of the former. We know of no substance in nature, that is impervious to that subtille fluid; it seems to pervade *all bodies* with nearly the same facility as caloric. Therefore, though snow indicates electricity, it is probably no more than it has acquired in its passage through an electrified atmosphere.

If those who are much more competent to the task than myself, would direct their attention to this most interesting branch of natural philosophy, I am inclined to think, they would find the result of their enquiries highly gratifying. Meteorology cannot be considered, but as yet in a state of infancy; for the greater part, our knowledge of it is hypotheses, which we cannot support by experiment. Therefore, it is only by a close observation of facts, accompanied by just inferences drawn from them, that we can arrive at any degree of certainty on this complicated subject.

IV.

Medico Chemical Researches on the Virtues and Principles of Cantharides. By H. BEAUPOIL.†

Experiments and
observations on
cantharides.

THOUGH the animal kingdom presents us with only a small number of substances of use in medicine, it must nevertheless be admitted, that among those we possess, there are some of which the effect is so certain, so constant, and so definite, that if we were deprived of them, it would be difficult, and perhaps impossible, to find others to supply their place.

Cantharides are of this number; their mode of action is universally known, together with the advantages they afford in many disorders. It is not, therefore, to be wondered that the

* The water gives out heat in congelation. *Vide* Irwine, Black, Crawford, &c.

† *Annales de Chimie*, XLVIII. 29.

examination of these insects should have engaged the attention of celebrated physicians, and that their analysis should have been frequently attempted by chymists. Experiments and observations on cantharides.

The principal aim of all those who have operated upon this material, has been to discover whether the blistering property which it so eminently possesses does generally appertain to all the parts of the animal, or whether it do not rather reside in some peculiar matter, which, independent of the parts which accompany it, is capable of acting alone, and producing the effects which are observable by the entire cantharides.

It is undoubtedly needless to repeat in this place, what has been said and done respecting this object; but it is essential to remark, that no one before Thouvenel pursued that course which could lead to the solution of the problem offered for consideration; and accordingly, we must consider the period in which that physician published his experiments on cantharides, as the earliest time in which philosophers could indulge the hope of ascertaining some positive information respecting the nature and properties of the immediate material of those insects.

But while we render justice to the labours of Thouvenel, we must confess that he has not carried them to an extent answerable to his happy commencement. For he has neglected some of the most important questions, and among others, those which relate to the vesicatory, diuretic, and aphrodisiac properties of cantharides.

It was to supply in some measure the deficiency of that respectable philosopher upon the above three points, that Citizen Beaupoil has thought fit to undertake a new examination of cantharides. The paper in the annals consists of an extract from his memoir, by Citizen Deyeux.

The author divides his dissertation into four parts.

In the first he gives a rapid sketch of the specific properties of cantharides; the methods used for collecting them, and the preparations to which they are subjected previous to their introduction to the market as an article of commerce.

In the second he gives a slight history of the use and application of these insects from the time of Hippocrates to the present period.

In the third we find an accurate outline of the attempts made by chymists to analyze the cantharides, as well as an account of his own particular experiments and their results.

The

Experiments and
observations on
cantharides.

The fourth contains every thing which relates to the physiological essays made with these animals; the effects produced by their exhibition, whether internally or externally; and lastly, observations relative to the opening of the bodies of several dogs, to which the author had given either the entire cantharides, or the different immediate materials, which he separated by means of his processes.

As the first and second parts contain nothing more than is to be met with in various authors, it is unnecessary to attend to them, and accordingly, the abridger has confined himself to the third part, in which the chemical facts are given.

Thouvenel, who was the first rational experimenter on cantharides, made use of water and alcohol to separate the soluble parts of these insects, and the results he obtained were,

1. A yellow reddish extractive matter, of a sharp bitter taste, resembling, as he says, that of ants, but less acid.

2. Another yellow matter, of a paler colour than the former, and nearly insipid.

3. A fatty matter, of a green colour and acrid taste, possessing the smell by which the entire cantharides are distinguished.

4. Lastly, A parenchymatous matter.

Citizen Beaupoil obtained similar products; but he not only ascertained their existence, but examined them separately, and in this it is that the difference between his labours, and those of Thouvenel principally consists.

He first observed that the aqueous solution of the peculiar extractive matter afforded by cantharides, does not fail to undergo a kind of alteration when exposed to the air; that the fluid becomes turbid, affords a yellowish precipitate, which acquires a peculiar odour; becomes covered with a viscid pellicle, emitting a fœtid ammoniacal smell; and that when it has arrived at this term, the fluid no longer exhibits any sensible change. He afterwards remarked that the solution here mentioned before it undergoes those changes which are produced by exposure to the air, strongly reddens the tincture of turnsole; that when mixed with rectified alcohol or ether, it becomes divided into two parts, nearly equal; the one possessing the form of a black adhesive precipitate, insoluble in alcohol, and the other that of a yellow brown matter, very soluble in that fluid.

(The Conclusion in our next.)

Description

V.

Description of a Galvanic Apparatus affording a large Surface for Oxidation, and convertible into one or more Plates at pleasure. By I. R. I.

To Mr. NICHOLSON.

SIR,

Edinburgh, May 10, 1804.

I AM much flattered by the notice that you and Mr. Wilkinson have been pleased to take of my late communication, and I think I have done no small service to science by drawing forth from both of you, the very ingenious disquisitions towards the œconomy of galvanism, which appeared in your last Journal.

Your description of what may be called a Polychrest trough, Improvement in the galvanic trough. seems to reach as near perfection, in every requisite property for giving the shock, or deflagrating metals, as could be wished: Yet there is still a further improvement which has suggested itself to my mind; but as it is probable the waste of the zinc would be more considerable, I mention it with diffidence.

It appeared to me in some experiments I made with the *coronne de tasses*, that the metals were more corroded to produce the same effect, than happened either in the pile or the trough; but if Mr. W.'s idea is just (which is most likely to be, from his great experience in the science), that the effect is in proportion to the oxigenation of the metal, the following plan appears the best adapted for both œconomy and power. Corrosion greatly in the *coronne de tasses*.

In Fig. 3, Plate VI. A represents, by a side view or section, Description. a plate of zinc of six inches by three; in its centre is a square piece, from which rises another piece of a smaller diameter, either square or round. These projecting bits are both sunk in a block of wood somewhat larger than the whole plate, but not so as to pass through it; and the plate must stand clear of the wood and every part of the cell, hanging entirely by the knob, and strongly cemented. A bore is made down through the thickness of the wood, in a line with one through the small knob of the zinc.

The copper wire B, passed through that hole, is the medium of communication from cell to cell; and the same mode may be

be employed in forming them into one plate, as suggested, in your last, when a deflagrating power is required.

The intermediate parts express the wooden blocks, which must be well covered with cement on every side, and inserted into grooves, as the zinc and copper plates usually are.

The engraving that referred to my last communication, was inaccurately explained, as the dotted work was said to express the zinc, whereas the acid liquor was denoted by it.

I remain, Sir,

With much respect,

Your obedient servant,

I. R. I.

VI.

On the Difference between the Effects of Electricity and of Heat.

By Cit. BERTHOLLET.*

Difference between electricity and caloric.

I HAVE thought it important to determine the difference which exists between the action of the electric fluid and that of caloric, and the cause which renders their effects similar, more especially, as in the lessons of the Normal schools this similitude of effect made me adopt the opinion of those who have considered the electric fluid to be caloric itself; I consequently requested permission of Citizen Charles to make use of his powerful apparatus in the experiments which appeared to me to be necessary on this subject. With that civility which those engaged in similar pursuits are always sure to experience from him, he undertook to perform them himself. I now give the result, such as it was communicated to me by Guy Lussac, who assisted in the experiments.

Platina was not much heated by an electric shock nearly capable of burning it.

A wire of platina was submitted to shocks which were nearly strong enough to effect its combustion; and to be satisfied of this, a shock was excited by which a great part of the wire was melted and dispersed; afterwards the shocks employed were a little weaker, and immediately after each the wire was touched to judge of the temperature it had acquired: a heat was felt, which was dissipated in a few minutes, and which,

* From his *Essai de Statique Chimique*.

at the utmost, was estimated to resemble that of the boiling point of water. If electricity liquefied metals and brought them into combustion by the heat it excites, the platina wire must, after a shock which differed but little from that which would have produced its dispersion and its combustion, have approached the degree of temperature which occasions its liquefaction: now this degree, which is the most elevated that can be obtained, would, according to the valuation, more or less accurate, of Wedgwood, be 3227° of Fahrenheit.

When the shock is sufficiently strong to destroy the aggregation of the platina wire, it begins by detaching molecularæ from its surface, which exhale like smoke; if it is strong enough to produce combustion, the remains of the wire appear to be torn into filaments.

Electricity detaches the parts of platina, but does not fuse it.

A thermoscope, blackened with ink, and placed in the stream of a strong electric spark, only experienced a dilatation which was nearly equal to one degree of Reaumur's thermometer; and this slight effect might depend on the oxidation of the iron of the ink: placed beside the current, it did not shew any dilatation, although the air was necessarily affected by the electric action: it was the same when it was placed in contact with a metallic conductor which received a stream less powerful than in the preceding experiments.*

The electric current of sparks causes little heat.

A cylinder of glass filled with air, with an exciter at each of its extremities, to one of which was fixed a tube, communicating with another cylinder filled with water, produced an impulse at each shock which raised the water more than a decimeter above its level; but its effect was instantaneous.

Electric shock through air.

These experiments seem to me to prove that electricity does not act on substances, and on their combinations, by an elevation of temperature, but by a dilatation which separates the molecularæ of bodies. The slight heat observed in the platina wire is only the effect of the compression produced by the molecularæ which first experience the electric action, or which experience it in a greater degree; it must therefore be compared to that excited by percussion or compression.

Hence it is inferred, that electricity acts not by heating, but dilating.

If the dilatation was the effect of heat, that experienced by a gas in the experiment related above, would not have been

* A small thermometer in the luminous current between two balls of wood, is raised 32 degrees.—Nairne.

instantaneous, it would only have experienced a progressive diminution by cooling, as when its expansion is owing to heat.

When ammoniacal gas is decomposed by electricity, it remains cold.

In the experiment by which ammoniacal gas is decomposed, the gas indubitably receives the electric action, and nevertheless it is not heated; and as soon as the decomposition is finished, its volume remains unchanged, because the electric action which is employed in this experiment, is not sufficiently energetic to cause a perceptible dilatation. No sensible dilatation is produced in a gas by a shock which is not very strong, because the impulse not being gradual, like the expansion arising from caloric, and being excited instantaneously, the resistance of the liquid becomes too great, and cannot be overcome unless the dilatation has great energy.

Lead exploded in azote continues metallic, and is not fused.

An experiment of Dieman and his learned associates confirms this explanation: They caused a shock to pass through lead placed in a vessel filled with azote gas, which could not oxidate it; it was reduced into powder retaining all its metallic properties: If it had experienced a liquefaction similar to the action of heat, it would have cooled gradually, and would have congealed into one, or at least into several masses.

No heat is produced by the electric dilatation of metals: the ignition and heat are from oxidation or combustion.

When a metal is submitted to the electric action, the effects produced immediately by the electricity must be distinguished from those which are owing to its oxidation: The first are limited to the diminution or destruction of the effects of the force of cohesion, to removing and dispersing the molecules; (if by this a little heat is disengaged, it is only owing to the compression sustained by some of the parts); but those which are occasioned by the oxidation, produce a high degree of heat, and then the effects assume all the appearances of an ordinary combustion; hence it arises that the most oxidable metals are those which become red with the greatest facility, and which most shew the properties of a metal liquefied by heat.

Electricity favours oxidation by diminishing cohesion.

Electricity favours this oxidation, in as much as it diminishes the force of cohesion: it is thus that an alkali renders the action of sulphur on oxygen much more powerful, by destroying the force of cohesion opposed to it, and that a metal dissolved in an amalgam is oxidized much more easily than when it is in a solid state. It is only by destroying the effects of the force of cohesion that heat itself produces the oxidation of metals, but the expansive action of electricity will have a great advantage

age over that of caloric, because its action is confined to a solid which it encounters in its course, so that the gas will not experience a dilatation in opposition to the contraction which accompanies the combination: To this circumstance may be applied what is observed in the action of oxygen gas, which is capable of completely reducing an oxide of iron placed in the focus of a burning glass, although whose two elements receive the heat equally, is deduced by this metal.

It is probable that it is also to the expansive effect of an electric current established between two metals, having a stratum of water interposed, that the oxidation observed by Faraday between these substances, placed in contact in water, is due, and which, in this case, appears to be confined to the reduction of the oxygen which is held in solution in this water.

The chemical effects produced in substances submitted to the action of electricity, seem capable of being deduced from general considerations, and of being explained by the diminution of the force of cohesion, which is an obstacle to the combination which their molecularæ tend to form; but the differences may be offered by positive electricity and negative electricity remain to be determined: the chemical effects of the Volta may be much more considerable than those of common electricity, although the latter possesses a much higher tension, because its action being necessarily interrupted, the chemical effects which require time to be accomplished, can only begin to be effected, and may even be destroyed, by the sudden re-establishment of the first state of the system, while the permanence of the action of the electromotor apparatus, although weaker at each instant, may give rise to chemical changes which it promotes, by diminishing the force of cohesion.

This explanation of the agency of electricity is general, but does not shew the difference between plus and minus.

Galvanism may owe its greater energy to its constancy.

I do not myself consider the explanations I have now hazarded as more than conjectures, which observation may confirm or destroy.

* Journ. de Phys. Vendem. An. X.

VII.

Letter from a Correspondent I. R. I. explaining some Facts in Galvanism, and on other Objects.

To Mr. NICHOLSON.

SIR,

Edinburgh, May 19, 1804.

Observations on galvanism.

I SENT you, a few days ago, some hints towards a farther improvement of the galvanic apparatus *, and I hope that the following observations will arrive in time to be added as a supplement to my last communication.

There seem to be two very marked laws in galvanism; the first is, that the phenomena are produced by moist oxidation, which seems a general one; but, by the second law, that they remain latent till one part of the conducting metal be made perfectly dry: this is at least strictly correct where two metals are employed.

From the tendency of galvanic effects to become latent, by the use of moist conductors, a pile cannot be formed unless one side of the oxidated metal be covered from moisture by cement, besides having a conductor of a different metallic substance. The trough is, therefore, best adapted to economize galvanism, as I have shewn, that nearly as large a surface can be exposed to the action of the fluid in it, as in the *couronne de tasses*, over which it has many advantages of steadiness, portability, &c.

From the necessity of dry conductors to alternate with moist to produce the more striking and perfect galvanic effects, it would appear that the former give the necessary celerity to their action; it might therefore be useful to enquire, whether making them of a considerable length, and inclosing them in some substance that would insure them from the moisture inseparable from the galvanic apparatus, might not considerably increase that celerity.

It is much to be wished that the end conductors were improved; jointed wires seem to lose much of the power, by not being in perfect contact; spiral wire seems better adapted to that use, and more capable of varying in its direction.

* See page 79.

It does not seem impossible to take a shock of its full force from piles or troughs of different powers, by bringing their conductors very near, but keeping them from actual contact by pieces of ivory or baked wood: it does not seem so clear that the full power of combustion could be preserved in the same manner.

Observations on galvanism.

I was lately rather surprized to find that galvanism had the effect of making silver remarkably brittle: this looks as if its action was somehow connected with the malleability of the perfect metals, and to shew that the conducting metals should be occasionally passed through the fire: the silver plates were nearly clean.

I will conclude with an observation on another subject. Having kept a considerable variety of Reeves's water-colour cakes in a damp closet, many of the cakes were in some degree affected by it so as to acquire mold; but the colour that Reeves sells under the name of Royal Smalt, has a most surprising power of attracting moisture, as it was reduced most completely to a soft mass; as almost all the vegetable, animal, and mineral substances used in painting were in the same box, we may pretty fairly conclude that an oxide of cobalt surpasses them all in its power of attracting moisture, and would make a most delicate hygrometer if suspended from one arm of a fine beam.

Attraction of moisture by oxide of cobalt.

I am, Sir,

With much respect,

Your obedient servant,

I. R. I.

VIII.

On the Presence of a new earthy Phosphate, found in the Bones of Animals, which does not exist in those of Men. By FOURCROY and VAUQUELIN. Read before the National Institute.†

WE flatter ourselves the Institute will recollect our laborious essays on the analysis of urine, and stony concretions found in the body of man and animals; with the striking difference existing between them, and the cause to which the latter is

Recapitulation of the analysis of urine, &c. No phosphate in the urine of animals,

• Gehlen's New Journal of Chemistry, I. 555.

owing.

owing. We endeavoured to prove in those essays that none of the salts called phosphates, (which exist in abundance in the urine of man) are to be found in the urine of mamiferous animals, that their kidneys are not the concretory of these saline combinations; but that the hair which covers their skin, and the corneous appendages which defend their extremities, are the organs and repositories in which nature secretes and deposits these salts in the body of animals.

Nor the morbid concretions called urinary calculi.

We have also shown that the morbid and preternatural concretions, called urinary calculi of man, contain besides uric acid, also phosphate of lime and phosphate of magnesia; none of which are met with in those of animals; and that on the contrary, the stony concretions found in the intestinal canal of animals, always contain phosphates of different kinds, whereas the concretions met with in the intestines of man, do not contain a vestige of them.

The bones also differ. Those of animals contain phosphate of magnesia; those of man do not.

We shall now endeavour to prove that the bones of animals also differ in composition from those of men. We have found that the former contain, besides phosphate and carbonate of lime, also phosphate of magnesia; the latter of which has hitherto escaped the notice of chemists. This salt which we have detected in the bones of all the animals we hitherto examined, does not at all exist in the bones of man. We shall first state the method we employed for detecting and separating it, and then point out the relative proportions in which it exists in different animals.

Method of separating magnesia from the bones of animals.

Take a quantity of bones of adult animals, burn them to whiteness in an open fire, and reduce them to a fine powder. Upon one part of this powder, after having been put into a convenient earthen or glass vessel, affuse an equal quantity by weight of concentrated sulphuric acid; stir the mixture intimately together, and then suffer it to stand for five or six days. Having done this, dilute the mass with at least ten times its bulk of water, agitate it well and transfer it on a strainer. When no more fluid passes, diffuse the mass again through five times its quantity of water, and strain again as before, and repeat this process till the water runs tasteless. The fluids thus obtained, are to be added together, and mingled with liquid ammonia, taking care the latter be in excess. The precipitate which is obtained consists of phosphate of lime, phosphate of ammonia, and phosphate of magnesia. To separate these

these salts, wash it in as little cold water as possible, and boil it in a solution of potash so long, till the odour of ammonia is no further perceptible. The potash in this process decomposes the phosphates of ammonia and magnesia, leaving the latter and phosphate of lime. To separate these two, let the whole be boiled in acetic acid, the magnesia will be dissolved, and the phosphate of lime remain untouched.

Method of separating magnesia from the bones of animals.

To obtain the magnesia, let the solution of acetite of magnesia be carefully evaporated to dryness, re-dissolve the salt in water, and decompose the solution by carbonate of soda; the precipitate obtained is carbonate of magnesia. If it be pure carbonate of magnesia, it will be completely soluble in sulphuric acid; if it contains lime, the solution will be cloudy, and a precipitate will gradually be deposited.

Such is the method we employed for detecting and separating the magnesia contained in the bones of animals; it is perhaps tedious, but it is easy and certain.

The bones of the ox examined in this manner yielded something less than $\frac{1}{8}$ of its weight of sulphate of magnesia, which is equal to about $\frac{1}{14}$ of phosphate of magnesia, or in the burnt bone to $\frac{1}{14}$.

The bones of the horse and sheep afforded $\frac{1}{8}$ of phosphate of magnesia.

Those of fowls and fish yielded nearly the same quantity as those of the ox,

The results of a general analysis of the bones of the ox were:

Dry gelatine	-	-	-	-	51	0
Phosphate of lime	-	-	-	-	37	7
Carb. of lime	-	-	-	-	10	0
Phosphate of magnesia	-	-	-	-	1	3
					100	0

The presence of phosphate of magnesia in the bones of animals, and its total absence in those of man, calls upon the physiologist to point out the source whence this salt is derived in the former, and why it is not met with in the latter. That it forms a constituent part of the food of both, we have proved elsewhere; that phosphate of lime enters into the composition of wheat, barley, oats, peas, &c. Why then is it not to be found in the bones of men? The nature of the human urine may perhaps assist in explaining this problem. We have proved that

that the urine of men contains phosphate of magnesia, and that the urine of animals is free from it. This salt is therefore ejected in man, by the kidneys; it therefore does not enter into the composition of bones; moreover the urinary calculi of man frequently contain phosphate of ammonia and magnesia, but no such salts are ever found in the stony concretions of the intestinal canal.

IX.

On the Nature of Oxygen, Hydrogen, Caloric, &c. as deduced from Galvanic Experiments. By A CORRESPONDENT.

On the existence of oxygen.

SINCE we are told that oxygen is one of the most essential substances in the production of the most striking phenomena of nature; that heat and light result from its change of combination, nay, that animal life is dependant on it, it becomes a matter of very great utility to investigate its nature, much more to prove whether it has any existence. It is well known that this substance, which has now so long and so generally been admitted, has never yet been exhibited except in combination, and that the evidence of its existence in combination has never been more than presumptive. It appears from some late experiments to be possible to substitute known for unknown principles, and to relieve science from those agents which are merely hypothetical.

Oxygen and hydrogen taken to be water with the power from each respective end of the galvanic wire.

The wires from the galvanic combination of metals produce different effects when placed in the same vessel of water. One produces inflammable air, the other vital air. When the circuit is made by the human body, a shock similar to an electrical one is perceived. It therefore appears that those powers which thus affect the human body, change water into inflammable and vital air. One wire always produces one air, and the other wire another air from water. Is it not therefore philosophical to refer the production of one of these airs to the power proceeding from one end of the pile and water; and the production of the other air to the power proceeding from the other end of the pile and water? In this experiment we are made sensible of no other principle, power, or substance than the above-mentioned. Why should we therefore have recourse to two hypothetical substances, oxygen and hydrogen, which have

have never in any experiment been made sensible to us? Is it not philosophical to refer phenomena to causes which are objects of our senses, rather than to account for them by agents which are merely hypothetical?

Under certain circumstances water is converted into two ^{Development of the facts and inferences.} airs, which airs have peculiar properties; in the galvanic experiment we are made acquainted with no other agent but those powers which are elicited by the particular arrangement of metals, and these powers we are made sensible of; they produce different effects on various substances, and therefore I contend that these powers are different agents: for the same powers under the same circumstances should produce the same effects. The zinc side of the galvanic arrangement produces vital air, whilst the copper side produces inflammable air.— Does it not appear from this experiment that there are other causes besides caloric that give aeriform elasticity to bodies? And do not the two powers of the pile here seem to be real principles? Each of them produces a real and decided effect on water. At all events we have not in this experiment any reason for asserting that water is a compound body, formed of two distinct and solid substances, oxygen and hydrogen. I do not mean at present to inquire whether negative electricity be a mere negation or not. We know that it is as much a cause of repulsion as what is called positive electricity, and that in experiments of a different kind from the one we are now considering. When a substance has in one instance been clearly proved to be formed of certain principles, it is consistent with philosophical accuracy to refer in all other instances to the same principles as the causes of the production of such substance. Inflammable air therefore is water rendered aeriform by negative electricity or galvanism; and vital air is water rendered aeriform by positive electricity or galvanism. This is nearly the enunciation of the fact, and I contend that in this, and in all the reasonings respecting water and fire, we have no occasion for the two hypothetical principles, oxygen and hydrogen.

In the above experiment with the pile of Volta, it appears ^{The effect not caused by caloric.} that it is not caloric which causes the elastic aeriform state of either the vital or the inflammable air, at least we are not made sensible of it. If it were caloric that proceeded from the two wires of the pile, why should each wire uniformly produce the same air, and one different from the other? There should be

be some reason why the solid base hydrogen attaches itself to the caloric of the one wire, and why the solid base oxygen is always so ready to enter into combination with the caloric of the other wire.

Compound nature of heat.

Scheele has asserted the compound nature of the matter of heat, and that all inflammable bodies contain a principle of inflammability, which principle, by combining with his igneous air, produces the phenomena of combustion. It does not appear that this opinion has been controverted by any decisive fact. We now know that some principle besides caloric is necessary in the production of one inflammable body, namely, of inflammable air; which principle, by combining with vital or igneous air, produces combustion. The re-produced water is common to both.

Generation of fire.

I think upon a further prosecution of this inquiry, it will appear, that fire is generated during combustion, that it is fire alone, (that is, the principle which causes the sensation of heat) that causes all the phenomena of combustion, and that it is, as Scheele observes, the water of his igneous air which forms the additional weight of bodies after they are burned. I know of no chemical fact that contradicts this explanation, in which no new substance that is not sensibly discovered is introduced.

A CORRESPONDENT.

X.

*Experiments on the Yolk of Wool, followed by some Considerations on the Cleansing and Bleaching of Wool. By CIT. VAUQUELIN.**

Yolk of wool.

SEVERAL philosophers have thought that the yolk of wool was a fatty matter; others from its dissolving in water could not adopt the same opinion. Chemical analysis alone could decide this question, and this is what I proposed to myself in the work, the result of which I now offer.

Action of water upon wool.

1st. Water deprives wool of much of its colour, and the liquid acquires colour, odour and taste.

2d. The washings of the wool is milky like an emulsion of gum-resin, and passes through paper with difficulty.

* From the Ann. de Chimie, Fructidor, An. XI. No. 141.

3d. In time it gives a deposition of sand, carbonate of lime, and several other foreign bodies; it lathers by agitation and heat like a solution of soap.

4th. The water with which wool has been washed, filtered and evaporated, yields a brown extract, thick like a syrup, of an acrid, salt, and bitter taste: in this state it still retains its peculiar odour. Aqueous solution and extract.

5th. Alcohol, applied to this extract, dissolves a part which communicates a reddish-brown colour to it: if the alcohol be separated from this substance by evaporation, it assumes the form of a transparent, thick, and viscous honey. Extract exposed to alcohol.

The following are some of the properties which it offered; To acids.

1st. It dissolves easily in water, and its solution is speedily coagulated by the acids, which separate a fat substance from it, insoluble in water. The matter thus separated by the acids, collects very slowly; its colour is yellowish. The acids, as will be seen lower, retain a great quantity of it in solution, which gives them a reddish brown colour. By evaporation, the greatest part of this substance, dissolved by the acids, is deposited in the form of a black bitumen, and salts are obtained with base of potash and of lime. The greasy matter is so adherent to these salts that they cannot be obtained in a state of purity and whiteness, until after several calcinations and solutions. It yields salts with base of lime and potash.

At the same time that the acids precipitate this fat matter, they drive off a certain quantity of acetous acid, very distinguishable by its odour. Concentrated sulphuric acid blackens the inspissated yolk, and disengages some vapours of muriatic acid. Acetous acid, and muriatic acid.

2d. Lime-water renders the solution of the yolk turbid and milky, but it does not form a coagulum in it as in a solution of common soap. Lime-water.

3d. Caustic alkalis or quick-lime do not demonstrate the presence of ammonia. Caustic alkalis and quick-lime.

4th. Nitrate of silver produces a yellow precipitate in it, which attaches itself to the sides of the vessel, like a fat substance. Great part of this precipitate is dissolved in nitric acid. Nitrate of silver.

The part of the yolk which is insoluble in alcohol has still a salt taste, but less distinct than the part which is soluble in this re-agent. After having been thus treated with alcohol, it does not entirely re-dissolve in water; there remains a glutinous matter, The insoluble part is still salt.
Is not entirely soluble in water after having been treated with alcohol.

Contains an alkaline carbonate.

Action of reagents.

matter, of a grey colour, with which the acids produce a pretty brisk effervescence, which shews the presence of an alkaline carbonate. The portion which retains its solubility in water communicates a reddish colour and a saline taste to this fluid; its solution is not disturbed by the acids, as it was before having been treated by alcohol. Caustic alkalis do not disengage any ammonia; the muriate of barites forms a very abundant deposition in it, the greatest part of which is soluble in water: the nitrate of silver also occasions a precipitate in it, which dissolves partly in nitric acid. Alcohol precipitates this matter in the form of a mucilage, which is deposited quickly.

Nitrate of iron being mixed with the solution of this substance, formed a brown precipitate in it, and at the end of some days, the liquor furnished a pretty large quantity of nitrate of potash.

The yolk being decomposed by dilute sulphuric acid, and the liquor filtered, it blackened by evaporation, exhaled vapours of sulphuric acid, and became carbonaceous, as the concentration of the sulphuric acid took place. The residue being afterwards washed with water, and the solution suitably evaporated, yielded crystals of neutral sulphate of potash, but a good deal remained in the solution on account of the superabundant acid which brought it to the state of an acidulous salt: by a longer evaporation, this salt crystallizes in needles and plates of a pearly white.

Yields sulphate of lime by sulphuric acid;

During the course of these successive evaporations, another species of salt was offered, in the form of flattened needles, of a fatty white, and without any sensible taste.

This salt examined with care, appeared to me to be only sulphate of lime; it however differed from it in some respects: for example, it melts much more readily by the flame of the blow-pipe into a globule, transparent while it is in fusion, and which becomes opaque by contracting: it is also much more soluble in water, and nevertheless does not contain the acid in excess; as I have satisfied myself. Its solution in water precipitates muriate of barites and oxalate of ammonia abundantly: one of these precipitates is sulphate of barites, and the other oxalate of lime. Neither lime-water nor ammonia disturb its solution. It appears therefore that this salt is a modification of sulphate of lime, which is probably produced by the proportion of its elements. It may also be possible that this salt still contains

tains some portions of fat matter, which, by decomposing the sulphate of lime, and forming a little sulphuret would facilitate the fusion. I regret my not having had a sufficient quantity of this salt, to examine its properties more minutely.

The yolk dissolved in water, filtered, and inspissated, having Acetic acid; been distilled with dilute sulphuric acid, furnished a liquor in which I easily recognized acetic acid, by its odour, its taste, and the properties of the salts which it formed with different bases, particularly with lime and potash.

Thus the yolk contains acetic acid, which without doubt is combined with part of the potash.

It also contains muriate of potash, for, with the solution of ^{and muriatic} silver it forms an abundant precipitate, which is not entirely acid. soluble in nitric acid; and, by distillation with sulphuric acid, it gives sensible indications of muriatic acid, which is mixed with the acetic acid.

The yolk evaporated to dryness, and strongly heated in a silver crucible, swells, chars, and exhales fetid ammoniacal vapours; afterwards oily fumes arise which take fire, and when the greatest part of the oil is dissipated, it reddens, and enters into quiet fusion. If, at this moment, it be poured on a marble, it yields a substance which contracts by cooling, of a greyish colour, and a very caustic alkaline taste: if this substance be afterwards dissolved in water, there only remains an infinitely small quantity of carbonaceous matter, and, by evaporation, the liquor yields a true potash slightly carbonated.

It results from these experiments that the oil or grease, ^{Recapitulation of the component parts:} whose presence in the yolk has been demonstrated by means of the acids, is combined in it with potash, in the state of a true animal soap; that, besides, there is a portion of carbonate of potash in excess, since the acids produce a pretty brisk frothy effervescence in the concentrated solution of the yolk. In addition to the substances which I have just mentioned, the yolk contains a certain quantity of animal matter; for, by distillation, it gives very sensible traces of ammoniac, and an oil whose fetid odour resembles those furnished by animal matters.

The yolk is therefore formed, 1st, Of a soap with a base of potash, which makes the greatest part; 2^d, Of a small quantity of carbonate of potash; 3^d, Of a perceptible quantity of acetate of potash; 4th, Of lime, whose state of combination

I am

I am unacquainted with ; 5th, Of an atom of muriate of potash ; 6th, finally, Of an animal matter to which I attribute the peculiar odour of the yolk.

which are not accidental.

I am of opinion that all these matters are essential to the nature of the yolk, and are not found in it by accident ; for I have constantly found them in a great number of samples, as well of Spanish wool as of French.

I do not here speak of the other matters, insoluble in water, which are also met with in wool, such as the carbonate of lime, sand, and filth of every sort, these being evidently accidental.

Are they the products of cutaneous transpiration ?

It remained now to enquire if all the matters in the yolk were the product of cutaneous transpiration, accumulated and thickened in the wool, or if they were taken up in the folds and other places in which the sheep lie. It is very certain that all the elements fit for the formation of the matters contained in the yolk, are found in the excrements of these animals, and in the vegetables which serve them for litter. Nevertheless, I could not believe that all of it was the effect of dung ; on the contrary, I am of opinion that the humour of the transpiration is the principal source of it.

The analysis of the dung offers nothing certain in this respect, because the matters found in it may have been deposited there by the sheep themselves.

In what state are they emitted by the skin ?

But admitting that the principles of the yolk arise from the cutaneous transpiration, which is very probable, are these matters emitted by the body of the animal in this state, and do they not experience some change while they remain in the wool ? This is a question on which it is difficult to decide positively ; we can only presume that changes are produced in it, as in all very complex substances deprived of motion, of which, in the present case, we neither know the cause nor the manner.

Washing the wool in running water is not enough to cleanse it.

The yolk, as we have seen above, being a true soap, soluble in water and alcohol, it would seem that nothing better can be done for scouring the wool than to wash them in running water. But I should observe that there is a small quantity of fat matter in the wool, which is not in combination with the alkali, and which, remaining attached to the wool, keeps it a little glutinous (*poisseux*), notwithstanding the most careful washing.

But

But if the wool be put into buckets, and only as much water as will moisten it poured in, and if it be suffered to remain some time in this bath, pressing it often, it scours much better, and becomes much whiter afterwards, by washing in running water.

The scourers have a custom of macerating the wool in putrefied urine, and it is generally believed that it is the ammonia which is developed that effects the scouring; but I have some reason to think that this alkali is of no value. This effect is rather owing to the yolk itself, or to some other principle of the urine, to the urée, for example. The following are the grounds of my opinion in this respect; I put wool washed in running water into a mixture of sal ammonia and common potash; the mixture had a strong smell of ammonia, and nevertheless the wool was in no respect cleansed, because this alkali does not form, or at least with great difficulty, a saponaceous combination with the greasy matter of wool. From these observations, therefore, I believe putrid urine to be nearly useless in the scouring of wool, at least as far as respects its ammonia.

Putrid urine does not promote the scouring.

Though the utility of putrid urine be in some degree doubtful, it is, on the contrary, very certain that fresh urine would be greatly injurious to the proposed object, for the soap contained in the yolk would incontestibly experience a decomposition by the acid of the urine, which would precipitate the grease on the wool.

Fresh urine would precipitate the grease.

I suspect that the same effect would take place from washing the wool in water containing earthy salts, which are known to decompose alkaline soaps. For which reason it is always prudent to employ the purest water which can be procured for this purpose.

As would water containing earthy salts.

This is not the case with soap-suds, which accomplish the scouring of wool perfectly, at the same time giving it more whiteness. It, therefore, after having washed the wool in running water till it loses no more, it be suffered to macerate for a few hours in only one twentieth of its weight of soap dissolved in a sufficient quantity of warm water, squeezing it often, it will be entirely purged of the small portion of grease which still adhered to it, and will then have a softness and degree of clearness which it could not have had without this operation.

Soap-suds the best menstruum.

The

The yolk exercises an action on the uncombined grease.

Danger of continuing its action too long; or of using strong soap-suds.

The acrimony of the yolk probably hurtful to the living animal.

Washing recommended.

Loss of weight by scouring.

Bleaching of scoured wool.

The yolk itself, when a little concentrated, as I have already mentioned, has an efficacious action on the portion of grease which is not in a saponaceous state; for I have found that, in putting to the wool only the quantity of water necessary to cover it, it scours better, particularly with a little heat, than when it is washed in running water. But I also found that, when wool has remained too long in its own yolk, it swells, splits, and loses its strength; this effect also takes place with soap-suds which are too strong.

Since the solution of the yolk occasions this swelling and splitting of the wool, is it not possible that this accident may happen on the sheep's back, particularly in hot, moist seasons, or when they are shut up in folds in which the litter is not often enough renewed? Nor would it be impossible that the acrimony of the yolk should occasion an irritation in their skins, and, by that means, be the cause of some of the disorders to which this organ is liable in these animals, which must principally happen in hot and damp weather: fortunately, in these seasons, they are from time to time exposed to rains which wash them, and carry off at least a portion of this matter. On this subject I cordially agree with those who think that washing sheep in hot and dry weather, would be useful to their health and the quality of their wool.

The loss experienced by scouring wool is very variable; the greatest I met with was 45 per cent, and the least 35; it is true, those which I washed were very dry. This loss is not wholly owing to the yolk; the humidity, the earth, and the filth of every species, also contribute to it.

I have made some attempts to bleach scoured wool, but I confess that they have not been carried so far as they ought to have been. I have remarked, generally, that those which had been washed with soap-suds whitened better, by every method, than those which had not. Sulphureous acid dissolved in water whitened it pretty well, but it did not destroy the yellow colour which the wool, growing in the groin and under the fore-legs of the sheep, had contracted. In liquid sulphureous acid the wool acquires the property of crackling between the fingers like brimstoned silk, and, at the same time, contracts a very powerful fetid smell, which is not dissipated in a long time.

I did

I did not try the vapour of burning sulphur, but all the world knows that it whitens wool well, and that the woollen manufacturers use it to give the finishing degree of whiteness to their goods. Of all the methods which I tried, I found none better for bleaching the wool, than exposing it, on the grass, to the dew and the sun, after being well scoured with weak soap-suds: the yellow spots of that from the groin, however, were not entirely destroyed; they had only diminished in intensity.

XL.

Copy of a Letter from Mr. CUTHBERTSON to Dr. PEARSON, communicating an important and curious distinguishing Property between the Galvanic and Electric Fluids. Communicated by Dr. PEARSON.

To Dr. PEARSON:

DEAR SIR,

I THINK it right to inform you, that yesterday evening I resumed the experiments with the galvanic batteries; the result was—

1. Charcoal was deflagrated and ignited for about one inch in length.
2. Iron wire $\frac{1}{16}$ inch diameter, was melted into a ball $\frac{1}{16}$ inch diameter.
3. Platina wire $\frac{1}{16}$ inch diameter, was melted into a ball $\frac{1}{16}$ inch diameter.
4. Brass wire $\frac{1}{16}$ inch diameter, $\frac{1}{2}$ inch in length was ignited.
5. Ditto $\frac{1}{16}$ inch diameter, was red-hot at the extremity.
6. Iron wire $\frac{1}{16}$ inch diameter, was red-hot for 16 inches in length.
7. Ditto 12 inches, deflagrated and melted into a ball.
8. Ditto six inches in length, were deflagrated.
9. Ditto 8 inches in length, were ignited.

Two troughs, each trough containing 30 pair of plates six inches square, were used for the first seven experiments, and one of these troughs only for the two last experiments.

VOL. VIII.—JUNE, 1804.

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The

Galvanic deflagrations.

The four last experiments prove, I think, that *double quantities of galvanic fluid only burn double lengths of wire, and not the square, as electrical discharges do* *.

I am, with the greatest respect,

Sir,

Your very humble servant,

JOHN CUTHBERTSON.

Poland-Street, Soho,

March 27, 1804.

XII.

Letter from a Correspondent, containing an Observation of the spontaneous Inflammation of Paper in Nitric Acid Gas.

To Mr. NICHOLSON.

DEAR SIR,

London, May 22, 1804.

Paper inflamed
in nitrous gas.

THAT several different inflammable bodies, while in a state of inflammation or of ignition, burn with an enlarged flame, and continue ignited when immersed in nitric acid gas, is, I suppose, commonly known; but that paper itself would take fire and flame most beautifully in this gas, and at not a very elevated temperature, has not, that I recollect, been already observed. By the following accidental circumstance this phenomenon was seen this morning in the public lecture-room, while reading on the subject of nitrous acid. In putting together the different parts of the Wolfe's apparatus, having ready only a bent tube much smaller than the lateral aperture of the globe condensing receiver, I filled up that aperture, partly with a piece of writing paper which projected into the receiver, and partly with almond paste. Soon after the acid had begun to distil, and while the apparatus was filled with

* It is not said whether the two troughs were used collaterally or longitudinally. Electrical jars may be considered as being always combined in the former mode. This subject seems to require comparison with the facts given in Mr. Wilkinson's letter in our Journal, Vol. VII. p. 207; but the communication came too late for me to offer any remarks upon it.

W. N.

reddish coloured nitrous acid gas, I was surprized by the bursting forth of flame from the paper, which was consumed by it in less than a minute, without cracking, as I expected, the receiver. I think it unnecessary to make any comments, or give the *rationale* of this fact.

Always yours faithfully,

AMICUS.

XIII.

Description of a Jib on a new Construction; by Mr. J. BRAMAN, Engineer. Communicated by the Inventor.

JIBS of the usual construction turn on two solid gudgeons. The rope by which the goods are raised, passes over the upper gudgeon, and is confined between two small vertical rollers, in order that it may constantly lead fair with the pulley or sheave at the extremity of the jib. According to this construction, whenever the crane turns round its axis, the rope is bended so as to form an angle more or less acute, which causes a great increase of friction, and produces a continual effort to bring the arm of the jib into a parallel position to the inner part of the rope. These inconveniences may appear to be trifling on paper, but in actual practice they are of no small importance, for they necessarily imply a much greater exertion of power in raising goods, and the application of a constant force to keep the jib in the position that may be requisite; while the partial stress which is exerted on only a few strands of the rope, when bended into an acute angle, infallibly destroys it in a very short time.

Description of a jib on a new construction.

The simple construction exhibited in Plate V. obviates all these defects, and at the same time possesses the very desirable property of permitting the jib of what is termed a camphut or landing crane, wholly to revolve round its axis, and to land goods at any point of the circle described by the arm of the jib.

It consists in perforating the axis or pillar of the crane, and in conducting the rope through this perforation by means of an additional pully fixed on the top of the arm of the jib.

Description of a
jib on a new
construction.

The nature of the contrivance cannot fail to be sufficiently understood by an inspection of the figures; the one of which represents a jib attached to the wall of a warehouse, the other a campshut or landing crane fixed on the edge of the wharf. Each of these jibs turns on a perforated axis or pillar. The rope proceeds from the goods which are hoisted, through a pully fixed as usual at the extremity of the jib; it then passes over another pully fixed at the opposite extremity of the jib, and is, by this pully conducted through the perforated axis or pillar to a third pulley; whence it is immediately directed to the crane by which the weight is elevated.

It is almost unnecessary to state that the lower axis is usually fixed in an oil box, and that friction rollers are applied to the axis wherever the circumstances may render it necessary.

The importance of this improvement, in an article of such extensive use, must be evident even to those who are the least acquainted with the subject. Mechanics who are aware that simplicity of construction and certainty of effect are among the most valuable characters to be sought in engines, will most probably observe this crane with pleasure; and the advantages to the community at large must be measured by the convenience and saving of labour it is calculated to afford.

XIV.

A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake, and other American Serpents. By BENJAMIN SMITH BARTON, M. D. From the American Transactions, Vol. IV.

(Concluded from Page 62.)

Other snakes (particularly the black-snake which is not poisonous) are said to cha.m.

SECONDLY. It is a fact well known in this country, that the rattle-snake is not the only kind of serpent that is said to be endued with the faculty of fascinating birds, squirrels, and other animals. As far as my inquiries have extended, it does not appear to me that, in general, the rattle-snake is thought to have so large a portion of this faculty as some other species of serpents. Of this, at least, I am certain, that persons residing in our country-situations tell us many wonderful tales of the bewitching eyes of the black-snake, the coluber constrictor of

of Linnæus, as they do of the boiquira, or rattle-snake. Now let it be supposed, for a minute, that the poison of this latter serpent, when thrown into the body of a bird, a squirrel, &c. is capable of producing, in these animals, those piteous cries, those singular movements, those tremulous fears, which are mentioned by Kalm, by de la Cépède, and by other writers,—in what manner are we to account for the similar cries, movements, and fears, in those birds which are frequently seen under the fascinating influence of the black-snake? For we Americans all know, that the bite of the black-snake is perfectly innocuous. This, indeed, is also the case with the greater number of the species of serpents that have, hitherto, been discovered in the extensive country of the United States. And yet almost every species of serpents is supposed to be endued with the power of fascinating such animals as it occasionally devours.

These facts, and this mode of reasoning, certainly involve, in some difficulty, Mr. de la Cépède, and those writers who espouse his opinion, which I have examined, under the first head of my objections. An attempt is made to account for the imaginary fascinating faculty of the serpent from the powerful influence of a subtle poison. But, upon inquiry, it is found, that the power of bewitching different animals is not an exclusive gift of those serpents which nature has provided with envenomed fangs: it is a gift which as extensively belongs to that more numerous tribe of our serpents, whose bite is innocent, and whose creeping motion is their only poison *.

and consequently
the supposition
of poisonous in-
fluence is un-
founded.

These

* If there is any impropriety in this mode of expression, the impropriety has its source in my feelings, with respect to the serpents. Perhaps, no man experiences the force and the miseries of this prejudice in a greater degree than I do. It is the only prejudice which, I think, I have not strength to subdue. As the natural history of the Serpents is a very curious and interesting part of the science of zoology; as the United-States afford an ample opportunity for the farther improvement of the history of these animals, and as I have, for a long time, been anxious to devote a portion of my leisure time to an investigation of their physiology, in particular, I cannot but exceedingly regret my weakness and timidity, in this respect. I had meditated a series of experiments upon the respiration, the digestion, and the generation of the serpents of Pennsylvania. But, I want the fortitude which it is necessary to possess in entering on the

These objections will, I am persuaded, be sufficient to convince every unprejudiced reader, that the system of explanation offered by Mr. de la Cépède is unfounded in facts; and, consequently, that the problem still remains to be solved, in another way.

Professor Blumenbach admits the fact; but ascribes it to a moral cause.

Among the number of ingenious men who have amused themselves with speculations on the subject of this memoir, and who, rejecting the commonly received notion of the existence of a fascinating power in the rattle-snake, have attempted to explain the phenomenon upon other principles, it is with pleasure I recognize the respectable Professor Blumenbach, of Gottingen. This gentleman, in a late publication, speaking of the rattle-snake, makes a few remarks on the fascinating faculty which has been ascribed to this reptile. These remarks I shall translate at length.

Toads, hawks, &c. are said to have the power of fascinating by the eye.

"That squirrels, small birds, &c." says he, "voluntarily fall from trees into the jaws of the rattle-snake, lying under them, is certainly founded in facts: nor is this much to be wondered at, as similar phenomena have been observed in other species of serpents, and even in toads, hawks, and in cats, all of which, to appearance, can under particular circumstances, entice other small animals, by mere steadfast looks. Here the rattles of this snake (the rattle-snake) are of peculiar service; for their hissing noise causes the squirrels, whether impelled by a kind of curiosity, misunderstanding, or dreadful fear, to follow it, as it would seem, of their own accord. At least," continues Mr. Blumenbach, "I know from well-informed eye witnesses, that it is one of the common practices among the younger savages to hide themselves in the woods, and by counterfeiting the hissing of the rattle-snake to allure and catch the squirrels."*

the task. Instead of slowly and cautiously dissecting and examining their structure and their functions, with that attention which the subject merits, I am more disposed, at present, to obey the injunction of the Mantuan poet, in the following beautiful lines:

—Cape saxa manu: cape robora, pastor,
Tollentemque minas et sibila colla tumentem
Dijice: jamque fuga tumidum caput abdidit alte,
Cum medii nexus, extremæque agmina caudæ
Solvuntur, tardosque trahit unus ultimus orbes.

GEORG. Lib. iii. 420—424.

* Handbuch der Naturgeschichte, P. 253, Göttingen: 1791.

I do

I do not intend to take up much time in examining the foregoing explanation. I shall offer my objections to it, in as concise a manner as I can.

First. The faculty of fascinating is by no means peculiar to the rattle-snake, but is attributed as extensively to the black-snake, and other serpents, which are not furnished with the crepitaculum, or set of bells *, by which this serpent is supposed to be enabled to ring for its prey, when it wants it.

Examination of the facts stated by Blumenbach, which are contested.

Secondly. Some persons, who have seen the rattle-snake in the supposed act of charming, assure me that the reptile did not shake its rattles, but kept them still. It is true, that Mr. Vosmæer's rattle-snake, already mentioned, continually shook its rattles.

Thirdly. With regard to the practice of the young savages, spoken of by Mr. Blumenbach, I know nothing. I have inquired of Indians, and of persons who have resided for a considerable time, among the Indians, and they appear to be as ignorant of the circumstance as I am myself. I am inclined to think that Mr. Blumenbach has been imposed upon: or, perhaps, the following circumstance may have given rise to the story. The young Indians put arrows, across, in their mouths, and by the quivering motion of their lips upon the arrows, imitate the noise of young birds, thus bringing the old ones so near to them, that they can be readily shot at. In like manner, the Lanius Excubitor, or great shrike, hiding itself in a thicket, and imitating the cry of a young bird, often succeeds in seizing the old ones, which have been solicited, by the counterfeited noise, to the assistance of their young.

Facts and observations respecting the power of fascination ascribed to snakes.

Ever since I have been accustomed to contemplate the objects of nature with a degree of minute attention, I have considered the whole story of the enchanting faculty of the rattle-snake, and of other serpents, as destitute of a solid foundation. I have attentively listened to many stories, which have been related to me as proofs of the doctrine, by men whose veracity I could not suspect. But there is a stubborn incredulity often attached to certain minds. In me it was strong. The mere force of argument never compelled me to believe. I always suspected, that there was some deficiency in the extent of observation, and the result of not a little attention to the subject has taught me, that there is but one

* Serpent à sonnette is the French name for the rattle-snake.

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wonder in the business;—the wonder that the story should ever have been believed by a man of understanding, and of observation.

In conducting my inquiries into this curious subject, I thought it would be proper, and even necessary, previously to my forming a decided opinion, to ascertain the two following points, viz. first: what species of birds are most frequently observed to be enchanted by the serpents? and, secondly, at what season of the year has any particular species been most commonly seen under this wonderful influence? I was induced to believe that the solution of these two questions would serve as a clue to the investigation of what has been long considered as one of the most mysterious operations in nature. I am persuaded that I have not been mistaken. Possibly, the credulous may not think as I do.

It is a curious circumstance in the history of birds, that almost every species, in the same country at least, has an almost uniform and determinate method of building its nest, whether we consider the form of the nest, the materials of which it is constructed, or the place in which it is fixed*. Some observations on this subject are necessarily connected with the point under investigation, in this memoir;—indeed, they are involved in the question concerning the species of birds which have most generally been observed to be enchanted by the rattle-snake, &c.

Some birds build their nests on the summits of the loftiest trees; others suspend them, in a pendulous manner, at the extremity of a branch, or even on a leaf†, whilst others build them

* I do not mean, by this observation, to assert, that birds are necessarily impelled to construct their nests of the same materials, or to place them in the same situations; yet such is the language of some writers on natural history, and on morals, who talk of the “determinate instinct” of animals, and who think it impossible that “animals of the same species should any where differ.” “The grouse in America, we are told, perch upon trees; the hare burrows in the ground; and we have, in these instances, sufficient reason to deny that the species of either is the same with those of a like denomination, with which we are acquainted, in Europe.” These are the words of the celebrated author. See Dr. A. Ferguson’s *Principles of Moral and Political Science*, vol. i. p. 59 & 60. quarto edition.

† See a very interesting account of the *Motacilla futoria*, or
Taylor-

them on the lower branches, among bushes, and in the hollows of decayed, and other trees. Many species, again, are content with the ground, laying their eggs, and hatching them, in the cavity of a stone, an excavation from the earth, among the grass of fields and meadows, or in fields of wheat, rye, and other grains. Thus, to confine myself to our own country, the eagle, the vulture, the hawk, and other birds of this extensive family, make choice of the loftiest oaks, and other trees of our forests; the baltimore-oriole *, commonly called, in Pennsylvania, the hanging bird, suspends a beautiful nest to the extremity of a branch of the Liriodendron †, or some other tree; the migrating thrush ‡, called robin, is content with the lower branches; the red thrush §, the cat-bird ||, the red-winged oriole, called the swamp-black-bird ¶, and many others build in the low bushes; the wood-peckers **, the blue motacilla (blue-bird) ††, the torchepot ‡‡, and others, build in the hollows of trees, the chattering plover §§, and the whippoor-will ||||, take advantage of a hollow place in the ground, or in a stone, which the great lark ¶¶, the marsh-wren ***, &c. place their nests in the grass; and, lastly, the partridge ††† builds in the corn-fields.

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Of all these birds, and of a great many others, those which build their nests upon the ground, on the lower branches of trees, and on low bushes (especially on the sides of rivers, creeks, and other waters, that are frequented by different kinds of serpents), have most frequently been observed to be under the enchanting faculty of the rattle-snake, &c. Indeed, the bewitching spirit of these serpents seems to be almost entirely limited to these kinds of birds. Hence, we so frequently hear tales of the fascination of our cat-bird, which builds its nest in the low bushes, on the sides of creeks, and other waters, the most usual haunts of the black-snake, and

Taylor-bird, by my learned friend Mr. Pennant, in his Indian Zoology, pages 44, 45 & 46.

- | | |
|----------------------------|----------------------------|
| * Oriolus Baltimore. | † Liriodendron tulipifera. |
| ‡ Turdus migratorius. | § Turdus rufus. |
| Muscicapa carolinensis. | ¶ Oriolus phœniceus. |
| ** Pici | †† Motacilla Sialis. |
| ‡‡ Sitta. | §§ Charadrius vociferus. |
| Caprimulgus. | ¶¶ Alauda magna. |
| *** Motacilla Troglodytes? | ††† Tetrao virginianus. |

other

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other serpents. Hence, too, upon opening the stomachs of some of our serpents, if we often find that they contain birds, it is almost entirely those birds which build in the manner I have just mentioned.

This fact I had long remarked. It had made some impression upon my mind before I had turned my attention to the subject of this memoir. Lately, when I came to take a view of the subject, the fact appeared to me to be of some consequence. I shall now avail myself of it.

The rattle-snake seldom, if ever, climbs up trees*. He is

* Some respectable writers assert, that the rattle-snake does climb trees, and that it does it with ease. Mr. de la Cépède is of this opinion. After telling us that this reptile lives upon worms, frogs, and hares, this naturalist proceeds: "il fait aussi sa proie d'oiseaux & d'écureuils; car il monte avec facilité sur les arbres, & s'y élance avec vivacité de branche en branche, ainsi que sur les points des rochers qu'il habite, & ce n'est que dans la plaine qu'il court avec difficulté, & qu'il est plus aisé d'éviter sa poursuite." *Histoire Naturelle des Serpens*. p. 409. At the conclusion of his account of the boiquira, or *crotalus horridus*, the eloquent author has run into the same error, in the following beautiful, though rather poetical, apostrophe. "Tranquilles habitans de nos contrées tempérées, que nous sommes plus heureux, loin de ces plages où la chaleur & l'humidité règnent avec tant de force! Nous ne voyons point un Serpent funeste infecter l'eau au milieu de laquelle il nage avec facilité; les arbres dont il parcourt les rameaux avec vitesse; la terre dont il peuple les cavernes; les bois solitaires, où il exerce le même empire que le tigre dans ses déserts brûlans, dont l'obscurité livre plus sûrement sa proie à sa morsure. Ne regrettons pas les beautés naturelles de ces climats plus chauds que le nôtre, leurs arbres plus rous, leurs feuillages plus agréables, leurs fleurs plus suaves, plus belles: ces fleurs, ces feuillages, ces arbres cachent la demeure du Serpent à sonnette." *Histoire Naturelle des Serpens*. p. 419 & 420. I have been at some pains to discover whether the rattle-snake does climb up trees. The result of my inquiries is that it does not. Although I have had opportunities of seeing great numbers of rattle-snakes in the western parts of Pennsylvania, &c. particularly in the vicinity of the river Ohio, I never saw one of them except on the ground. The black-snake I have often seen upon trees. I ought not, however, to conceal that in the summer of the last year, a Choktah-Indian told me, that the rattle-snake does climb trees and bushes, to a small height. He said, that he had once seen one of these snakes upon a reed. I am not very willing

is frequently, however, found about their roots, especially in wet situations. It is said that this reptile is often seen, curled round a tree, darting terrible glances at a squirrel, which after some time is so much influenced by these glances, or by some subtle emanation from the body of the serpent, that the poor animal falls into the jaws of its enemy. This story is, I believe, destitute of foundation, though it is related by the good Cotton Mather*. The rattle-snake is, indeed, sometimes seen at the root of a tree, upon the lower branches of which, at the height of a few feet from the ground, a bird or squirrel has been seen exhibiting symptoms of fear and distress. Is this a matter of any wonder? Nature has taught different animals what animals are their enemies; and although, as will be afterwards shewn, the principal food of the rattle-snake is the great frog, yet as he occasionally devours birds and squirrels, to these animals he must necessarily be an object of fear. When the reptile, therefore, lies at the foot of a tree, the bird or the squirrel will feel itself uneasy. That it will sometimes run towards the serpent, then retire, and return again, I will not deny. But that it is irresistibly drawn into the jaws of the serpent, I do deny: because it is very frequently seen to drive the serpent from its hold; because the bird or squirrel often returns, in a few minutes, to their

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willing to deny this Indian's story: yet it is opposed to every information I have been able to procure from persons well acquainted with the reptile of which I am speaking. However, it is not impossible that where trees and bushes grow very close together, the snake may climb them to a very small height. Most species of serpents move in a spiral manner: the rattle-snake moves strait on; and this is the reason why he cannot climb trees. In the quotation which I have made from Mr. de la Cépède, another mistake is involved. He speaks of the agility with which the rattle-snake moves. This is not, however, merely the mistake of Mr. de la Cépède. We find it in Piso. Speaking of this reptile, our author says: "In triviis juxta ac devius locis cernitur, tam celeriter proreptans ut volare videatur, idque velocius per loca saxosa, quam terrestria." *De Indiæ utriusque re naturali et medica*. p. 274. Now the truth is that the rattle-snake is one of the most sluggish of all our serpents. Linnæus was well informed, when he asserted that Providence had given "the Crotalus a very slow motion." See *Reflections*, &c. quoted p. 84 of this memoir.

* Philosophical Transactions of the Royal Society, No. 339.

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habitations. Sometimes the bird or squirrel, in attempting to drive away the snake, approach too near to their enemy, and are bitten, or immediately devoured. But, from what will afterwards be said, it will appear that these instances are not so common as is generally imagined.

My inquiries concerning the season of the year, at which any particular species of birds has been seen under the fascinating power of a serpent, afforded me still more satisfaction. In almost every instance, I found that the supposed fascinating faculty of the serpent was exerted upon the birds at the particular season of their laying their eggs, of their hatching, or of their rearing their young, still tender, and defenceless. I now began to suspect, that the cries and fears of birds supposed to be fascinated originated in an endeavour to protect their nest or young. My inquiries have convinced me that this is the case.

I have already observed, that the rattle-snake does not climb up trees. But the black-snake and some other species of the genus *coluber* do. When impelled by hunger, and incapable of satisfying it by the capture of animals on the ground, they begin to glide up trees or bushes, upon which a bird has its nest. The bird is not ignorant of the serpent's object. She leaves her nest, whether it contains eggs or young ones, and endeavours to oppose the reptile's progress. In doing this, she is actuated by the strength of her instinctive attachment to her eggs, or of affection to her young. Her cry is melancholy, her motions are tremulous. She exposes herself to the most imminent danger. Sometimes, she approaches so near the reptile that he seizes her as his prey. But this is far from being universally the case. Often, she compels the serpent to leave the tree, and then returns to her nest*.

* Horace, though he has not, like his contemporary, Virgil, given any great proofs of his knowledge in natural history, appears to have known, full well, the anxiety of birds for the preservation of their young :

" Ut assidens implumis pullis avis

" Serpentium allapsus timet."

EPOD. 1.

The author of these two fine lines, had he lived in America, the land of fascination, would, I am inclined to think, have disbelieved, the whole story. They would have been a clue to light and truth on this subject.

It is a well known fact, that among some species of birds, Facts and observations respecting the power of fascination attributed to snakes. the female, at a certain period, is accustomed to compel the young ones to leave the nest; that is, when the young have acquired so much strength that they are no longer entitled to all her care. But they still claim some of her care. Their flights are awkward, and soon broken by fatigue. They fall to the ground, where they are frequently exposed to the attacks of the serpent, which attempts to devour them. In this situation of affairs, the mother will place herself upon a branch of a tree, or bush, in the vicinity of the serpent. She will dart upon the serpent, in order to prevent the destruction of her young: but fear, the instinct of self-preservation, will compel her to retire. She leaves the serpent, however, but for a short time, and then returns again. Oftentimes, she prevents the destruction of her young, attacking the snake, with her wing, her beak, or her claws. Should the reptile succeed in capturing the young, the mother is exposed to less danger. For, whilst engaged in swallowing them, he has neither inclination nor power to seize upon the old one. But the appetite of the serpent-tribe is great: the capacity of their stomachs is not less so. The danger of the mother is at hand, when the young are devoured. The snake seizes upon her; and this is the catastrophe, which crowns the tale of fascination!

An attachment to our offspring is not peculiar to the human kind alone. It is an instinct which pervades the universe of animals. It is a spark of the divinity that actuates the greater number of living existences. It is a passion which, in my mind, at least, declares, in language most emphatic, the existence, the superintendence, the benevolence, of a first great cause, who regards with partial and parental, if not with equal eyes the falling of a sparrow and the falling of an empire.

Among the greater number of the species of birds, the attachment of the parent to the young is remarkably strong. We have daily instances of this attachment among our domestic birds, and I believe, it is stronger among these birds in their wild state: for there are some reasons for suspecting, that this amiable instinct is diminished and weakened by culture*.

* This question will be examined in my memoirs upon the force, or affections, of animals.

Facts and observations respecting the power of fascination ascribed to snakes.

The instances which I have already mentioned, as well as a fact, which remains to be mentioned, point out, in a striking view, the attachment of the mother-bird to her offspring. She often guards her nest with the greatest attention, fearful of the insidious glide of the serpent. She endeavours to prevent the destruction of her eggs or young, by this enemy. When he has succeeded in obtaining them, she attacks him either alone, or calls other birds to her assistance. We ought not to be surprised, that sometimes she falls a victim to her affection. For it is a well known fact, that some species of birds will suffer themselves to be taken upon their nests, rather than relinquish their young, or their eggs.

In the study of natural history, I am always happy to discover new instances of the wisdom of providence, and new proofs of the strong affections of animals. And for the discovery of such instances of wisdom, and such proofs of affection, the contemplation of nature is an ample field. In the instances now before us, the strength of the instinct of affection in birds is illustrated, in a striking point of view; and I cannot help observing, that I feel an high degree of pleasure in being able to do away, in some measure at least, a prejudice, not less extensive than it is unfounded, by bearing my slender testimony in favour of the existence and the powerful dominion of a benevolent principle in animals.

The following fact was communicated to me, some time since, by our president, Mr. Rittenhouse. I think it strikingly illustrates and confirms the system which I have been endeavouring to establish. I relate it, therefore, with pleasure, and the more so, as I have no doubt, that the authority of a cautious and enlightened philosopher will greatly contribute to the destruction of a superstitious notion which disgraces the page of natural history.

Some years since, this ingenious gentleman was induced to suppose, from the peculiar melancholy cry of a red-winged-maize-thief*, that a snake was at no great distance from it, and that the bird was in distress. He threw a stone at the place from which the cry proceeded, which had the effect of driving the bird away. The poor animal, however, im-

* Commonly called, in Pennsylvania, the Swamp-Black-bird. It is the *Oriolus phoeniceus* of Linnæus.

mediately

mediately returned to the same spot. Mr. Rittenhouse now went to the place where the bird alighted, and, to his great astonishment, he found it perched upon the back of a large black-snake, which it was pecking with its beak. At this very time, the serpent was in the act of swallowing a young bird, and from the enlarged size of the reptile's belly it was evident, that it had already swallowed two or three other young birds. After the snake was killed, the old bird flew away.

Facts and observations respecting the power of insatiation ascribed to snakes.

Mr. Rittenhouse says, that the cry and actions of this bird had been precisely similar to those of a bird which is said to be under the fascinating influence of a serpent; and I doubt not that this very instance would, by many credulous persons, have been adduced as a proof of the existence of such a faculty. But what can be more evident than the general explanation of this case? The maize-thief builds its nest in low bushes, the bottoms of which are the usual haunts of the black-snake. The reptile found no difficulty in gliding up to the nest, from which, most probably in the absence of the mother, it had taken the young ones. Or it had seized the young ones, after they had been forced from the nest, by the mother. In either case, the mother had come to prevent them from being devoured.

We are well acquainted with the common food of the rattle-snake. It is the great-frog* of our rivers, creeks, and other waters. The snake lies insidiously in wait for his prey, at the water-edge. He employs no machinery of enchantment. He trusts to his cunning and his strength.

A very ingenious† friend of mine, who has devoted considerable attention to the natural history of the rattle-snake, and who has dissected many of them, assures me, that he never saw but one instance in which a bird was found in the stomach of this reptile, and this bird was the chewink, or ground-robin‡. In another instance, he saw a ground-squirrel§ taken out of one of these reptiles. In every other case, so long as the food retained enough of the form to be distinguished, the stomach was found to contain the great-frog, which I have mentioned.

* *Rana ocellato* of Linnæus. † Timothy Matlack, Esq.

‡ This is the *Fringilla erythrophthalma* of Linnæus.

§ The *Sciurus striatus* of Linnæus.

Facts and observations respecting the power of fascination ascribed to snakes.

Another argument against the fascinating power of the serpent-tribe still remains to be considered.

It is natural to inquire, for what purpose nature has endowed serpents with the supposed powers of fascinating birds, and other animals? The answer to this question is uniform. It is said, the power is given that the serpents may obtain their food. Let us examine this opinion.

Admitting the existence of this power, I should have no hesitation in believing, that its use is what is here mentioned, though, indeed, it ought not to be concealed, that snakes are supposed, by some foolish people, to have the power of charming even children. And yet, I believe, there are no instances recorded of our American snakes devouring children. If, then, nature, in the immensity of her kindness, had gifted the serpents with this wonderful power, we should, at least, expect to find that the common and principal food of these serpents was those animals, viz. birds and squirrels, upon which this influence is generally observed to be exerted. This, however, is by no means the case.

As connected with this part of my memoir, it will not be improper to observe, that all our serpents are the food of different kinds of birds. Even the rattle-snake, whose poison produces such alarming symptoms in man, and other animals, is frequently devoured by some of our stronger and more courageous birds. As far as I can learn, the birds which most commonly attack and destroy this reptile, are the swallow-tailed hawk*, and the larger kinds of owls. The owl often feeds her young with this snake, whose bones are frequently found in her nest, at considerable heights from the ground. Even a hen has been known to leave, for a minute, her affrighted chickens, and attack, with her beak, a rattle-snake, the greater part of whose body she afterwards devoured†.

* *Falco furcatus*.

† It is commonly believed, that the rattle-snake is a very hardy animal; but this is not the case. A very small stroke on any part of its body disables it from running at all; and the slightest stroke upon the top of the head is followed by instant death. The skull-bone is remarkably thin and brittle; so much so indeed, that it is thought that a stroke from a wing of a thrush or robin would be sufficient to break it.

The

The black-snake is a serpent of much more activity than the rattle-snake. The latter, as I have already said *, seldom, if ever, climbs up trees. But the former will sometimes ascend the loftiest trees, in pursuit of the object of his appetite. The rattle-snake, it has been just observed, subsists principally upon the large frog, which frequents the waters of our country. He has, therefore, but little occasion for activity. But the black-snake, feeding more upon birds, stands more in need of activity. He frequently glides up the trees of the forest, &c. and, commonly in the absence of the mother, devours either her eggs or her young ones. The difficulty of obtaining his prey upon the tree is sometimes very considerable, as will appear from a fact which will be related immediately. Now, if this serpent is gifted with the faculty of fascinating, why is he not content to continue at the bottom of the tree, and bring down his object? And if he can employ this machinery of fascination at his pleasure, how comes it, that he so seldom succeeds in capturing old birds? For it is a fact that when birds are found in his stomach, they are principally young birds.

Facts and observations respecting the power of fascination ascribed to snakes.

I have said, that the black-snake sometimes finds great difficulty in obtaining his prey upon the tree. In support of this assertion I could adduce many facts. But my memoir has already exceeded the limits which I originally prescribed to it. I shall content myself, therefore, with relating a solitary fact, which strikingly illustrates my position.

A black-snake was seen climbing up a tree, evidently with the view of procuring the young birds in the nest of a Baltimore-bird. This bird, it has been already observed, suspends its nest at the extremity of the branch of a tree. The branch to which the bird, of which I am speaking, had affixed its nest, being very slender, the serpent found it impossible to come at the nest by crawling along it: he, therefore, took the advantage of another branch, which hung above the nest, and twisting a small portion of his tail around it, he was enabled, by stretching the remainder of his body, to reach the nest, into which he insinuated his head, and thus glutted his appetite with the young birds.

The importance of this fact, in the investigation of the subject of my memoir, appears to me to be great. An

* See page 106.

Facts and observations respecting the power of fascination ascribed to snakes.

American forest is not the silent residence of a few birds. During the greater part of the spring and summer months, our woods are alive with the numerous species of resident and visitant birds. At these times, if the black-snake possesses the faculty of fascinating, it cannot be a difficult thing for him to procure his food. Yet, in the instance which I have just related, we have seen this reptile climbing up a tree, and there obliged to exert all his ingenuity to obtain his prey.

I cannot well conclude this memoir without observing, that in the investigation of the subject which it involves, I have experienced much pleasure. For to the cultivators of science, the discovery of truth must, at all times, be a source of pleasure. This pleasure will even rise to something like happiness, when, in addition to the discovery of truth, we are enabled to draw aside the veil, which, for ages, has curtained superstition and credulity. Under the influence of various species of superstition, we fall from our dignity, and are often rendered unhappy. It should be one of the principal objects of science to rear and prop the dignity of the mind, and to smooth its way to comforts, and to happiness. The ills and the infirmities of our earthly state of being are numerous enough. It is folly, if not vice, to increase them. He who seriously believes, that an hideous reptile is gifted, from the sacred source of universal life and good, with the power of fascinating birds, squirrels, and other animals, will hardly stop here. He may, and probably will, believe much more. He will not, perhaps, think himself entirely exempted from this wonderful influence. He may suppose, that the property belongs to other beings, besides the serpents; and he will, perhaps, imagine that it forms a part of a more extensive plan, the effects of which, he will assert, are prominent, and unequivocal, though its ways, he will confess, are incomprehensible to mortal minds.

Historia naturalis non bene digesta abit in fabulam; præjudicia vero et nimia credulitas Veritatem, etsi cominus satis cognitam, longissime aliquando propellunt.

JACOBUS THEODORUS KLEIN.

Some

XV.

Some Account of an Egyptian Lock of very high Antiquity. Indicated from DENON's Travels, by a Correspondent. With Observations by W. N.

To Mr. NICHOLSON.

SIR,

A FEW weeks ago I saw, with particular pleasure, a model, Letter concerning a lock shewn at the Royal Institution. I beg leave to propose the insertion of the same in your excellent Work, and should hope for your opinion as to its security.

I am, Sir,

Your obliged reader,

New Broad Street,

P. Q.

May 12, 1804.

I HAVE given engravings of this lock, copied from Denon's book. *Plate VII.* shews the lock as applied to a door, and its developement is made in *Plate VIII.* The passage in Denon (translated) is as follows:

"No. 2, 3, 4, and 6. The Egyptian lock. It secures the gates of towns, of houses, and the apertures of the smallest articles of furniture or use. I have placed it among the antiquities, because it is the same as was in use four thousand years ago. I found one sculptured among the bas-reliefs which decorate the great temple of Karnack. It is simple in conception, easy of execution, no less sure than any other lock, and deserves to be applied on all our rural occasions. Fig. 2 is the key, which is capable of thousands of different combinations. In Fig. 3 the lock is shewn closed, seen as to its interior; the key being in the act of lifting up certain pins which had fallen into holes in the bolt, and kept it in its place. In Fig. 1 the bolt is drawn back, and the lock consequently open." See *plate 139 of the said work.*

Eton, in his Survey of the Turkish Empire, mentions this lock as follows:

"Nothing can be more clumsy than the door-locks in Turkey, but their mechanism to prevent picking is admirable. It

Description of the lock,

sculptured on the great temple of Karnack.

Eton's account of the same lock as of universal use in Turkey, &c.

It is a curious thing to see wooden locks upon iron doors, particularly in Asia, and on their caravanaries and other great buildings, as well as on house doors. The key goes into the back part of the bolt, and is composed of a square stick with five or six iron or wooden pins about half an inch long, towards the end of it, placed at irregular distances, and answering to holes in the upper part of the bolt, which is pierced with a square hole to receive the key. The key being put in as far as it will go, is then lifted up, and the pins entering corresponding holes raise other pins, which had dropt into these holes from the part of the lock immediately above, and which have heads to prevent them falling lower than is necessary. The bolt being thus freed from the upper pins, is drawn back by means of the key, the key is then lowered, and may be drawn out of the bolt: to lock it again the bolt is only pushed in, and the upper pins fall into the holes of their own weight. This idea might be improved on, but the Turks never think of improving."

Observations.

Simplest structure of this lock is a kind of latch.

The present lock.

Probable improvement.

This lock cannot be picked;

THE simplicity and other advantages of this lock or bolt, are too obvious to require much remark; for which reason I shall confine my present observations to its degree of security or inviolability. I think we may contemplate it in three several stages of perfection or improvement. 1. If it be constructed with one pin of considerable size to fall into the bolt, and the finger be supposed to be introduced for the purpose of raising it and setting the bolt at liberty, we shall have a fastening of nearly the same effect as the common latch. 2. Or if, instead of one falling pin, there be many, and an instrument be used to lift them, we shall have the bolt before us; and this, as far as we are informed, is the present state of the invention, though of so long standing. 3. Or thirdly, in case the present bolt should, on examination, be found to admit of being opened without extreme difficulty, it will become a question whether the principles of its structure can be so applied as to render it absolutely safe. This last question requires that we should first examine the ancient bolt a little more closely.

We may admit that the ancient lock, with many pins falling independently of each other, cannot be picked or opened without its key: and therefore we must ask whether the key can

can be made from an examination of the lock alone? In answer to this it may be noticed, that since all the pins in the key must be of equal height, the secret will consist in their relative distances and positions on the face of the key; and that these distances and positions can be easily known by introducing a stick, or key without pins, into the hollow of the bolt, and taking an impression (by means of a facing of soft clay) of the holes intended to receive the pins. After this a key may be made without difficulty; so that we arrive at our conclusion, that though this ingenious piece of mechanism cannot be violated without its key, yet it is easy to construct a key for that purpose.

but its key may be easily made from the lock.

We now come to the enquiry after that application of its principles which may render it absolutely safe. This is certainly possible; but not without considerably diminishing its simplicity. Besides several others, the following may be proposed: Let the dropping pin have an enlarged part, and a tail of wire above and below. Let the lower tail fall into its hole in the bolt, while the enlarged part falls into a socket made for its reception. Under these circumstances the bolt becomes fast: But when by raising the pin the enlarged part is clear of its socket, the bolt becomes free, and the lower tail is prevented from stopping it by a groove left for its reception. The bolt must have a covering piece of board, having a hole of the size of the enlarged part of the pin, directly above the socket into which it falls, and a groove for its upper tail; the interval between the covering-piece and the bolt itself being equal to the height of the enlarged part of the pin. By this means, when the pin is pushed up, just out of its socket, the bolt will move freely; but if it be pushed the least quantity farther, the enlarged part will enter the hole in the covering-board, and set it fast, as if it were in the socket; so that a very precise distance of elevation will be requisite. Lastly, the lower pin may be shorter or longer at pleasure. Now, if there be a number of these pins so placed and adjusted as to fall into their respective sockets at the position of the bolt when shut; if their lower tails be of different lengths, and a key be made to correspond with them, and lift them all to the proper height at once; the combination will be such as cannot be made out by any impression or tentative process upon the lock itself. For the evidence of a due length of any one

one of the lifting pieces of the key, will consist in the actual opening of the lock; and this cannot be had unless the due length of all the pins be obtained at once; against which the probability will be as the number of permutations of the pins, multiplied into the number of possible lengths of pins practically differing from each other in effect. Thus if the pins were six, the permutations would be $1 \times 2 \times 3 \times 4 \times 5 \times 6 = 720$; and if the length of a whole pin were one quarter of an inch, and a sensible difference would in practice arise from making the pins one-sixtieth of an inch longer or shorter, there would be 15 possible lengths for every pin. Whence $720 \times 15 = 10800$, the number of chances against the discovery.

W. N.

XVI.

On the Cause of the different Colours of the Triple Sets of Platina, and on the Existence of a new Metallic Substance in that Metal. By COLLET-DESCOTILS. - Presented to the Class of Mathematical and Physical Sciences of the National Institute of France.*

Precipitation of the solution of platina.

ALL chemists know that crude platina is easily soluble only in nitro-muriatic acid, that the solution is decomposable by muriate of ammonia and other salts with alkaline bases; and that the result of their former decomposition is a triple compound, consisting of oxid of platina, muriatic acid, and ammonia, or the alkali employed. The colour of this precipitate varies from a light yellow to a dark brown. It is sometimes also greenish. The latter is the case if the solution of platina is precipitated by a salt with base of soda.

Remarks.

Before I say any thing further concerning the causes which influence the colour of this precipitate, I shall point out some phenomena which characterise the solution of the metal itself.

Foreign admixtures in crude platina.

The grains of platina of commerce always contain more or less of foreign mixtures from which it should be previously freed as much as possible. The foreign bodies met with are most frequently minute stones, on which the acid employed for dissolving the metal, has little or no action, and two sorts of ferrugi-

* Gehlen's Chemical Journal, Vol. II. Part I. p. 73.

nous sand, one of which is obedient to the magnet, and another which is not attracted by the magnet, and which is only partially acted on by acids. I shall say no more in this place concerning these bodies, but that the first contains titanium and the second chromic acid, in considerable quantity.

The ferruginous sands contain titanium and chromic acid.

The best method to free platina of commerce from these admixtures, is that recommended by Proust, which consists in spreading out the platina on a sheet of paper, and carefully blowing away the lighter parts by means of a pair of bellows.

Mechanical purification by bellows.

Platina thus purified I introduced into a porcelain retort, to which a glass receiver had been fitted, previously filled one-third full of water. After having placed the retort in a reverberatory furnace, I raised the fire gradually, and increased the heat to the utmost I could produce, which was kept up for two hours. Nothing particular attended this process, except that the water with which the receiver had been partly filled, acquired a greenish hue towards the end of the process. On the roof of the retort, a fine blue powder was sublimed, of which I shall say no more at present than that it was soluble in water. The water in the receiver, after having been suffered to stand for a few days, had acquired a beautiful blue colour, which surpassed the colour of the best ultramarine.

Crude platina by violent distillation affords a blue sublimate soluble in water.

It was impossible to get the platina out of the retort. On breaking the distillatory vessel, the metal was found agglutinated, the upper surface of the mass had a rusty appearance, the middle was less discoloured, and the lower part had suffered no perceptible change.

The platina grains were agglutinated.

In order to examine the colouring matter which tinged the water of the receiver, I dropt into it a solution of an alkali; this produced instantly a blue precipitate. Sulphuric and muriatic acids, when mingled with this fluid, occasioned no change. Nitric and oxiginized muriatic acids changed this blue fluid, first to a lilac, but it soon lost this colour, and the whole became limpid. Water holding in solution sulphuretted hydrogen gas, occasioned no precipitate; but hidro-sulphuret of ammonia threw down a grey precipitate, which became blue by the affusion of acids, and then was rendered soluble again in hidro-sulphuret of ammonia.

The aqueous solution of the blue matter afforded a precipitate by alkali. Habitudes with acids, &c.

A small quantity of the blue precipitate collected from the roof of the retort, when urged with the blow-pipe in contact with borax, imparted no colour to this salt. When heated *per se*, it disappeared completely.

The blue sublimate disappeared before the blow-pipe: it did not colour borax.

Having

When platina is dissolved in n. m. acid, a black powder is separated.

Having so far proceeded, I made a solution of platina in nitro-muriatic acid (the platina had been freed from iron as much as possible by muriatic acid.) During this solution, a glittering black powder became separated, as is always the case when platina of commerce is dissolved in nitro-muriatic acid. If the operator be careful in collecting this powder as fast as it is separated, the quantity which may be collected, amounts to about 0,03 of the platina employed. But if this powder be not removed as fast as it is deposited in the solution, part of it becomes again acted upon, and a much less quantity is obtained.

Muriate of pl. was decomposed by mur. of ammonia.

The muriate of platina obtained, after having been suffered to repose and being decanted, I decomposed by adding by a saturated solution of muriate of ammonia; the precipitate was separated by decanting the supernatant fluid, and repeatedly washed till the water which passed, did not become green by the addition of prussiate of potash. The precipitate obtained was of a *yellow colour*. The decanted fluid from which the precipitate had been separated, and the first quantity of water employed for its ablution, after having been mingled together and concentrated by evaporation to one third of its bulk, were again mixed with a solution of muriate of ammonia; the precipitate now obtained was of a *dark red colour*. On treating the fluid separated from it, as before, the precipitate which did fall down was of a very *dark brown*. All these different coloured precipitates were carefully washed till they contained no vestige of copper or iron.

Yellow precipitate.

The decanted fluid being concentrated, and more muriate of amm. added, a *dark red precipitate* was obtained.

The fluid a second time decanted gave by mur. amm. a *dark brown precip.*

The colour of the precip. of platina is darker the more of the black powder in the solution.

I have remarked that if the solution of platina be slowly prepared, that is to say, if the platina be introduced into the nitro-muriatic acid, in small quantities at a time, and the subsequent solution of each quantity be respectively separated, and separately be decomposed by muriate of ammonia, the colour of the precipitate or triple salt obtained, is darker in proportion to the quantity of the black powder which was contained in the solution.

The black powder is (difficultly) soluble in nitr. m. acid; and decomp. by mur. of am.

This black powder which is deposited during the solution of platina of commerce * in nitro-muriatic acid, is soluble (though difficultly) in nitro-muriatic acid, composed of much nitric, and little muriatic acid; its solution is also decomposable by

* This black powder is likewise separated during the solution of *malleable platina* in nitro-muriatic acid.

F. A.
muriate

muriate of ammonia, and the colour of the precipitate is more or less intense, according to the quantity of powder contained in the solution.

From what has been so far stated, it appears, that this black powder is the cause of the different colours which the different precipitates or triple salts of platina exhibit, under different circumstances. The black powder is the cause of colour in the precipitates.

In order to learn the nature of this substance, I shall detail the experiments which were undertaken for that purpose. The precipitates or salts I made use of were, the triple ammoniacal muriate of platina, and the triple muriate of platina and soda; the former salt I preferred on account of its easy decomposibility, and the latter on account of being very soluble. Enquiry into the nature of this substance.

Experiments on the triple ammoniacal Muriate of Platina.

Equal quantities of the before-obtained yellow and dark red precipitate, being separately dissolved in equal quantities of water, the first salt furnished a solution of a gold yellow colour, whereas that of the latter was orange red. On adding to the latter solution a minute quantity of green sulphate of iron, or sulphureous acid, it became instantly of a gold yellow colour; the same effect was produced, though slowly, by the addition of alcohol. Aqueous solutions of the yellow and dark red precipitates; the first gold-yellow, the second orange red. The latter was rendered yellow by a sm. portion of gr. sulph. of iron, or by alcohol.

It was natural to suppose that the colour of the red salt might be owing to the higher degree of oxidizement of the dissolved platina. In order to convince myself of this conjecture, I attempted to transfer oxygen to the yellow salt, by means of nitric and oxiginized muriatic acids. This however failed; the colour of the salt remained yellow as before. On repeating the application of nitric and oxiginized muriatic acids alternately, the result was only a very pale red coloured precipitate. The red colour was not owing to greater oxidizement.

Equal parts of the yellow and red precipitate, deficcated at equal temperatures, and under the same circumstances, when decomposed by heat, yield also unequal quantities of fixed residue. That of the first, amounted to 0,44 of the weight of the salt employed, and that of the latter was 0,4 and 5. The yellow and red precipitates do not leave equal residues, when decomposed by heat.

If yellow precipitate be reduced by heat, the platina obtained is uncommonly soluble in a comparatively small quantity of nitro-muriatic acid, and the solution yield a yellow precipitate with muriate of ammonia. The yellow precipitate if reduced, affords very soluble platina; which again affords a yellow precip.

If

The red precipitate affords a less soluble metal which leaves a black powder, and affords a red precip.

If the red precipitate be reduced, the metal obtained is of a different nature from the former. It is far more insoluble in nitro-muriatic acid, and whatever quantity of acid may be applied, there remains constantly a quantity of black powder, which is absolutely insoluble in the acid. This solution, on being decomposed by muriate of ammonia, yields a red precipitate.

This precipitate treated with oxygenized muriate of potash with heat.

To learn the nature of this precipitate, I reduced a quantity of it, introduced it into a porcelain tube, connected with a small retort, containing hyper-oxygenized muriate of potash, and applied heat to the retort, after having first adapted to the other extremity of a receiver containing a little water. On increasing the heat so as to decompose the hyper-oxygenized muriate, the tube became lined with a blue powder, which was also observed in the empty part of the receiver.

Blue powder obtained;

After all the salt in the retort had been decomposed, I collected the blue sublimed powder. The minute quantity of it however did not permit me to submit it to many experiments. It was easily soluble in nitro-muriatic acid. Its nature will become more obvious hereafter.

—soluble in nitro-muriatic acid.

The red colour arises from a peculiar metal.

From what has been so far stated, it appears to follow that the red colour of the triple precipitate of platina, obtained by muriate of ammonia, or other salts with alkaline bases, is owing to the presence of a peculiar metal contained in the platina, which has hitherto been considered as simple.

Examination of the triple Muriate of Platina and Soda.

Triple muriate of platina and soda.

This triple compound is very little known.* It may easily be obtained by pouring into a solution of platina, a salt with base of soda. It is very soluble in water, and even in alcohol. The solutions are capable of crystallizing in long prisms, on three-sided tables, of a yellowish red colour. It is decomposable by muriate of ammonia; the precipitate is a muriate of platina and ammonia. It is likewise decomposed by a solution of soda; on adding this alkali in excess, the formed precipitate becomes again dissolved.

Reducible on charcoal, by the blow-pipe.

Muriate of platina and soda is reducible upon charcoal before the blow-pipe. The reduced metal possessed a considerable lustre.

* Muffin-Puschkin has pointed out some of its properties in Crell's Annales, 1800, Vol. I, p. 93. of which a short abstract is to be found in the Annales de Chimie. p. 277.

If crystallized muriate of platina and soda, free from all adhering or excess of acid, be exposed for some time to the air, its beautiful red colour becomes changed into a greenish hue. If the salt in that state be dissolved in water, and oxiginized muriate of lime be added to it, a dark brown precipitate falls, which after having been washed and dried, is soluble in muriatic acid, with which it forms a *beautiful blue solution*. This colour becomes again destroyed by the admixture of alcohol, and re-appears by the addition of oxiginized muriate of lime.

This precipitate is somewhat soluble in water; it is reducible when fused with borax, without imparting to the latter any colour. The reduced metal is very porous: it appeared not to be soluble in any of the acids.

If the solution of muriate of platina and soda, contain an excess of acid, it then is not disturbed by letting fall into it oxiginized muriate of lime; but on evaporation the mixture acquires a fine *green colour*.

Examination of the Yellow and Red triple Muriate of Platina.

If we add to the red triple muriate of platina, a solution of carbonate of soda, till it becomes completely dissolved the solution is of a yellow colour. On exposing it for some time to the contact of air, a green substance becomes deposited.

The yellow triple muriate of platina, treated in a like manner with carbonate of soda, yields a solution of a dark yellow, or orange colour, which suffers no change whatever on exposure to air.

The separation of the green substance from the red triple muriate, is much accelerated by the admixture of oxiginized muriatic acid. It seems as if the separation of this substance is owing to the action of oxygen; for no other acid is capable of producing it; at least the precipitate which they produce, is a mere oxid of platina.

The green substance may likewise be instantly obtained by evaporating the solution by heat.

If the solution of the yellow triple muriate be heated and evaporated, a very minute quantity of the green substance is indeed, sometimes deposited, and the remaining solution then acquires a more beautiful yellow colour. On continuing the evaporation of the yellow solution, a precipitate is deposited, which

Its crystals from a beautiful red, become green by exposure to the atmosphere. Its solution precip. by ox. muriate of lime.

Precipitate somewhat soluble; reducible into a porous metal insoluble in acids.

No precipitate if acid be in excess.

The red triple muriate becomes yellow by carbonate of soda; —and deposits a green precip. by expos. to the air.

The yellow thus treated becomes darker, and does not afterwards change.

The green deposit from the red triple muriate is from oxidization.

It is instantly had by evaporation.

The yellow triple muriate may afford a minute portion of green precip. by evap.—but this is an impurity. Its precip. is yellow.

which is not green, but *yellow*. This precipitate on being again dissolved in muriatic acid, and saturated with carbonate of soda, yields no farther green substance.

The green precipitate was reduced (by heat with borax) to a white brittle metal; difficultly acted on by nitro-m. acid;

more so when powdered.

The solution of the green substance became yellowish by sulphureous acid or green muriate of iron.

Ox. mur. of lime restored its green.

The green substance gave a blue sublimate, and left metal.

A portion of the same substance heated with nitre till the acid was decomposed. The alkali was dissolved in water, and not changed by acids.

The residue was not acted on by acids.

Hence chrome and molybdena were absent.

Separation of the platina from the triple muriate by alcohol.

I mingled a quantity of this green substance with vitrified borate of soda, and exposed the mixture for twenty-five minutes in a double crucible to the most intense heat I could produce; the result was a white brittle metal, which was difficultly acted upon by nitro-muriatic acid. The weak solution which had been obtained by this acid, had a violet colour; it yielded, on being evaporated, a dark green residue, which was soluble in muriatic acid, with which it formed a green fluid.

On pulverizing another quantity of this white brittle metal, and then exposing it to the action of nitro-muriatic acid, the solution was effected more easily, it was now more concentrated, and of a reddish-yellow colour. Muriate of ammonia let fall into it, threw down a little brownish-red precipitate, a proof that this metal still contained platina.

A quantity of the before-mentioned green substance being dissolved in muriatic acid, acquired a yellowish colour on being mingled with sulphureous acid, or with a solution of green muriate of iron. Oxygenized muriate of lime restored the original green colour.

A third part of the green substance, on being exposed to a violent heat in a porcelain retort, yielded a blackish-blue sublimate: the unsublimed part was a metallic substance, very difficultly soluble in nitro-muriatic acid.

A fourth portion of the green substance was urged, in a crucible head, with nitrate of potash. After the decomposition of the nitrate had been completed, the mass was diffused through water. The alkaline solution was colourless, and acids produced no change of colour in it. The residue, after repeated ablutions, was hardly acted upon by acids, the nitro-muriatic acid not excepted. This experiment precludes the presence of chrome and molybdena.

It is possible to separate nearly all the platina which is contained in the red triple muriate, by the mere admixture of alcohol, and the subsequent addition of dry potash or soda; for doing this much heat is evolved, and the reduction of platina takes place almost instantly. The same may be effected by means of carbonate of potash or soda; but in that case the solution must be highly concentrated. The reduction of the platina

platina then takes place even without the application of heat, but only after the space of some days.

The supernatant fluid of the reduced platina, when heated, acquires a lilac colour, which becomes blue on long exposure to air, depositing at last a greenish substance, resembling that hitherto treated of. Oxigenized muriatic acid favours the separation of this substance. The supernatant fluid affords the blue substance.

Sulphuretted hidrogen may also be employed for separating the platina from the red triple muriate; the platina becomes separated in the form of a brown powder; the other metallic substance remains undisturbed in the solution. It may, however, almost totally be precipitated by liquid ammonia. The precipitate obtained is brown. When fused with potash in a silver crucible, the mass acquires a green colour: On pouring muriatic acid on it, no complete solution could be effected; there always remained a powdery sediment which resisted likewise the action of nitro-muriatic acid. Carbonate of potash separated from this solution a small quantity of iron. The clear fluid from which the iron had been separated, remained perfectly transparent when heated; but it acquired a bluish hue, which increased on concentration, and after the exsiccation of the salt. On adding a little nitric acid to the salt, the blue colour became changed into a dark red. Sulphuretted hydrogen also separates the platina.

CONCLUSION.

I now flatter myself with having proved, by the above experiments, that the red coloured triple salts of platina owe their colour to the presence of a peculiar metal, oxidized to a certain degree. Conclusion. The red colour is owing to a peculiar oxide,

That this metal is nearly wholly insoluble in acids; that it becomes soluble in combination with platina; that when oxidized, it appears in the form of a blue oxide inclining to green; that its oxides, when combined with platina, are soluble in alkalies; that its acid solutions are not decomposable by sulphuretted hydrogen; that it imparts no colour to borax; that its oxides are reducible and volatilizable by heat, which volatilization becomes favoured by a stream of oxygen gas; and, lastly, that oxygen gas, assisted by heat, is capable of oxidizing this metal, and of volatilizing it in the form of a blue oxide. nearly insoluble in acids, unless combined with platina, &c.

These

The new metal
not yet named.

These properties do not characterize any of the known metals, and I am therefore authorized to consider it as a new one, to which I shall give a name when I have explored its nature more fully.

XVII.

Description of an Apparatus for filtering Water. By Messrs. HARMAN and DEARN, of Redriff.

Apparatus for
filtering water.

THE waters which run near or upon the surface of the earth, are usually contaminated by the remains of animal and vegetable substances in their progress towards entire decomposition, as well as by the minute powder of earthy or mineral bodies, which render it turbid and less fit for the purposes of domestic life. Spring or pump waters, by a natural filtration through the sandy strata of the ground, are mostly cleared from these mechanical admixtures; but in many places, as is the case with those of our metropolis, they are rendered impure, or, as it is called, hard, by an actual solution of sulphate of lime or plaster of Paris, which prevents their lathering with soap, and probably renders them less wholesome; besides which, they usually carry a portion of the drainage water in great towns, which renders them offensive at certain seasons, and at all times less worthy of confidence. For these and numerous other reasons, it has always been considered as a desirable object to clear waters, by filtration, from those impurities which render them less limpid, and a variety of apparatus have been offered to the public for that purpose.

In all these the process of nature has been imitated; namely, by causing the water to percolate either through sand or a sand-stone; the latter of which, though costly, seems at present to be almost the only method in use among us.

The contrivers of the simple and cheap apparatus delineated in Plate VI. are Messrs. Harman and Dearn, potters at Redriff, who remark, that the filtering-stone is not only expensive and liable to be clogged up and spoiled by the bodies deposited in its pores from the water, but that, as these bodies are actually in the progress to decay and decomposition, they are in some cases found actually to change the flavour and affect the purity of

of the fluid they are made use of to ameliorate. In consequence of which, they have been induced to apply their art to the simplest method of affording an apparatus for the filtering process, which shall not be liable to these objections. Apparatus for filtering waters

Plate VI. Fig. 1, represents the whole apparatus. Fig. 2 shews a shaded section. A is a vessel of stone-ware perforated with holes, *m*, at bottom, upon which coarse gravel, *h*, is laid, and upon that a stratum of fine gravel, and lastly fine sand, *g*. Or otherwise, the bottom may be covered with a coarse cloth, which will render the graduated fineness of the gravel and sand less necessary. Upon the top of the sand is laid a perforated and loaded board or plate of earthen ware, to prevent the sand from being disturbed when the water is poured in. B is a lower vessel, into which the filtered water from A drops, together with any sand that may escape from above. The clear water flows out through the neck *a c* into the vessel D for use.

The structure, uses, and effects of this apparatus are so obvious, that it is needless to enlarge upon them. The fineness and depth of the siliceous sand will regulate the perfection and expedition of the process; and the requisite cleanness and delicacy of the vessels and sand may be insured by changing the latter from time to time; for example, once in a fortnight or three weeks.

W. N.

XVIII.

Examination of a Stone containing Potash. By FREDERIC ACCUM, Teacher of Practical Chemistry, Pharmacy, and Mineralogy.

To Mr. NICHOLSON.

SIR,

SINCE Klaproth has detected potash in the lepidolite, leucite, and sonorous porphyry, chemists have sought for this alkali in other minerals, and their enquiries have not been disappointed: Tromsdorff has found it lately in the augite, and no doubt this alkali will be met with in many other minerals in which it was not expected. Being called upon by the company of potters of Staffordshire to examine a variety of Introductory letter.

of stones employed in the manufactory of earthen-ware, a particularly exact analysis was demanded of a stone, labelled *grey Cornish**, in which I detected this alkali. I shall not detain you with a circumstantial detail of my reiterated experiments, which were undertaken with a view to learn the nature of the stone under consideration, which is sought for by the potters with avidity; I shall merely confine myself to point out its characters, as well as that examination which may serve to establish the credit of my assertion.

I am, Sir, with respect,

Your most obedient humble servant,

FREDERICK ACCUM.

Old Compton Street, Soho.

Physical Characters of a Siliceous Stone containing Potash.

Physical characters of a stone containing potash.

THIS stone is found in amorphous masses, forming irregular strata, under the surface of a blue clay. Its colour, when newly taken from the bed, is a greenish-grey interspersed with black spots; but when left exposed to the air, it acquires an ash-grey colour. It is not very hard; its powder is white. When rubbed, it exhales a faint argillaceous odour. Its substance is coarse or uneven, having many small, sharp, abrupt, irregular elevations and irregularities. Its fracture is very irregular. It may be easily scratched with a knife. It scintillates with steel. It is absolutely opaque in small fragments. Urged before the blow-pipe, it froths and melts into a white enamel. Treated with borax in a similar way, it forms a reddish bead.

Its specific gravity is 2,465.

ANALYSIS.

Analysis of a stone containing potash.

Experiment 1.—One hundred grains of the finely levigated stone, after having been previously ignited†, were mixt with a solution of potash containing 400 grains of alkali, the mixture was evaporated in a silver capsel to dryness, transferred

* I am not permitted to state the exact place in Cornwall where this stone is found.

† 100 parts lost, during ignition, 6 parts,

into

into a crucible of silver, and fused for half an hour. The fused mafs had rather a pafly appearance, and could not be rendered perfectly fluid. Analysis of a
stone containing
potash.

Exp. II.—As foon as the crucible was nearly cold, it was removed from the furnace, its contents were softened by water, and the affufion of this fluid renewed from time to time, till all the fused mafs was detached from the crucible: about 18 times its quantity of water were expended for that purpofe.

Exp. III.—Into the obtained alkaline imperfect folution of muriatic acid was gradually poured, and the whole evaporated to drynefs.

Exp. IV.—The mafs was then transferred into a flask containing dilute muriatic acid, the whole was fuffered to boil for a few minutes, and the infoluble part feparated by the filtre. The filiceous earths thus obtained had a greyifh appearance, but it acquired a white colour after having been again digefted in muriatic acid, dried and ignited. It weighed 58 grains.

Exp. V.—The fluid from which this quantity of filex had been feparated, together with the muriatic acid employed for purifying it, and the water expended for ablution, were concentrated to about 20 cubic inches, and then mingled, boiling hot, with a folution of carbonate of foda; the precipitate deposited was collected, and washed as ufual.

Exp. VI.—As foon as the precipitate was fo dry that it could be removed from the filtre without lofing part of it, it was transferred into a folution of potafh, and the mixture boiled for about half an hour. On fuffering the alkaline folution to ftand undifturbed for twenty-four hours, a powder was deposited, which, on being again boiled in a more concentrated folution of potafh, remained unaltered. It was therefore collected, washed, dried, ignited, and put afide for further examination.

Exp. VII.—Into the alkaline folution, freed from this powder, I now dropt muriatic acid, till the precipitate which firft appeared again vanifhed, and then decomposed it by the addition of carbonate of ammonia. The precipitate obtained in this experiment, after repeated ablutions and ignition, amounted to 28 grains. Experiments not effential to be ftated here, convinced me that it was alumine.

Analysis of a
stone containing
potash.

Exp. VIII.—The precipitate of this experiment was dissolved in nitro-muriatic acid, and then mingled with liquid ammonia. A brown precipitate became deposited, which, after being dried, amounted to 0,25 grains. It was oxide of iron.

The fluid from which this iron had been separated, yielded no other products, on being carefully examined, except two grains of filix which escaped separation.

Thus far the stone had yielded siliceous earth, alumine and oxide of iron. I shall suppress those enquiries which proved fruitless for learning its farther composition, and state merely those which were attended with success for that purpose, and which were as follow :

Exp. IX.—One hundred grains of the stone reduced to an impalpable powder, were triturated with 600 grains of crystallized nitrate of barytes : the mixture fused till the flame of a piece of ignited wood did not become enlarged when held close over the surface of the fusing mass.

Exp. X.—The melted mass was softened with water, and digested in muriatic acid ; the fluid filtered, and the residue washed.

Exp. XI.—The liquid obtained in this process I neutralized with carbonate of ammonia, and added the latter till no further cloudiness ensued. After having separated the precipitate, I evaporated the fluid to dryness : the obtained salt I transferred into a glass tube closed at one end, and committed it to sublimation.

Exp. XII.—After the sublimation had ceased (which was known by a metallic wire introduced into the tube during the process, not becoming in the least covered with a coat of salt), I broke the tube and separated the salt. This being dissolved in water and crystallized, yielded nine grains of muriate of soda.

Exp. XIII.—To decompose this salt, I redissolved it in water, and mingled the solution with nitrate of silver till no farther cloudiness appeared : I then separated the muriate of silver formed, and decomposed the nitrate of potash by heat, which yielded 4,50 of potash,

The

The analysis being thus completed, I learnt that 100 grains of this siliceous stone from Cornwall contain,

Analysis of a
stone containing
potash.

Silex,	-	-	-	60
Alumina,	-	-	-	28
Oxide of iron,	-	-	-	0,25
Potash,	-	-	-	4,50
Water,	-	-	-	6
				98,75
Lofs,	-	-	-	1,25'
				100

XIX.

*Letter from A. CARLISLE, Esq. on the Temperature of
the Sea.*

To Mr. NICHOLSON.

DEAR SIR,

Soho Square, May 28, 1804.

THE following table was made by Mr. R. Perrins, Surgeon on the Honourable E. I. Company's establishment, during a voyage to Bombay in the year 1800. The temperatures in this table were noted at my request, from a desire to determine whether fishes possess any other temperature than that of the water in which they live, the negative being asserted by Linnæus. As, however, this imperfect journal may assist in similar researches, I beg leave to offer it for the use of your periodical work.

Table of the
temperature of
the sea, &c.

I am, Sir,

Your much obliged servant,

ANTHONY CARLISLE.

Table of the
temperature of
the sea, &c.

"Register of Atmospheric Temperatures, collated with those of
the Sea-Water taken on board the Honourable E. I. Company's
extra Ship Skelton Castle."

1800.	Atmo- sphere.	Sea-Water.	Lat. N.	Long. W.
Feb. 28	*	52°	42° 34'	15° 25'
March 2	*			
4	*	57	36 47	15
7	*	62	33 19	17 34
9	*	64	31 58	50
11	*	66	30 26	20 49
13	*	67	No observ.	59
15	71°	+68	26	23 24
16	72	69½	25 49	24
17	72	70	21 14	55
18	74	72	18 16	26 12
19	74	72	15 17	47
20	75	72	13 52	24 49
21	76	74	-11 58	23 6
22	78	+76½	9 46	21 12
23	80	78	7 43	20 11
24	82	80	5 25	20 13
25	82	80	4 24	20 6
26	84	82	3 33	19 41
27	84	82	2 58	19 27
28	85	82	2 35	19 17
29	86	83	2 7	19 10
30	86	83	35	20 1
31	85	82	33 S.	19
April 1	84	81	2 27	20 41
2	83	80	4 44	22 31
3	82	80	7 12	23 47
4	83	80	9 50	24 50
5	82	79	12 30	25
6	80	76	15 17	23 59
7	78	76	18 13	26 5
8	78	76	20 41	26 3
9	80	78	22 22	56
10	80	78	23 28	27 20
11	80	78	24 23	37
12	78	76 Shark 88†	24 48	32
13	76	74	26 29	22
14	74	72	27 50	24 14
15	72	70	28 42	35
16	72	70	31	45

* The atmospheric temperature was not set down during these days.

† Each trial upon the sea-water was repeated three or four times,
and the same results followed.

‡ Caught a shark, and found the heat in its stomach, after being
suddenly killed, 88°.

Table

[Table continued.]

1800.	Atmo- sphere.	Sea-Water.	Lat. S.	Long. W.	Table of the temperature of the sea, &c.
April 17	70°	68°	33° 10'	23° 57'	
18	70	66	34 20	22 46	
19	70	66	35 51	19 38	
20	68	66	36 2	18 35	
21	68	66	36 3	15 11	
22	68	64	35 56	11 22	
23	68	64		6 47	
24	66	64	36 47	3 15	
25	64	62	36 34	41 East	
26	64	62	39	3 40	
27	62	58	37 25	6 20	
28	61	58	53	7 40	
29	60	58	45	8 40	
30	60	58	36 30	12 11	
May 1	60	58	7	16 13	
2	60	58	30	18 13	
3	60	60	34	10 16	
4	60	60	37 22	22 25	
5	60	62	10	26 38	
6	58	64	36 54	50 51	
7	62	66	30	34 51	
8	64	66	37	39 39	
9	62	64	5	43 21	
10	62	64	20	45 21	
11	62	64	35 43	48 14	
12	64	66	33 15	49 38	
13	66	66	30 35	51 18	
14	68	68	27 35	52 12	
15	72	70	25 2	31	
16	76	74	21 21	39	
17	78	76	18 52	53 16	
18	78	76	16 25	54 12	
19	80	78	13 27	53 29	
20	78	78	12 41	50 55	
21	80	78	10 56	49 41	
22	80	78	7 8	49 41	
23	80	80	4 9	50 32	
24	81½	80	1 46	52 27	
25	84	81	13 N.	54 2	
26	86	82	2 32	50 59	
27	84	82	4 36	56 10	
28	85	82	9 26	60 51	
29	85	82	9 26	60 51	
30	85	82	14 31	64 41	
June 1	84	82	15 56	66 59	
2	86	82	17 39	69 40	
3	85½	84	18 57	72	
4	Arrived in Bombay Harbour.				

Observations

XX.

Observations upon the Doctrine of Count Rumford respecting the want of direct conducting Power in Fluids with regard to Heat. By CIT. BERTHOLLET.*

The doctrine of Count Rumford on the non-conducting property of fluids deserves examination.

COUNT RUMFORD has published several memoirs, by which he has endeavoured to prove that liquids and elastic fluids are not conductors of heat, and that they only transmit caloric by means of the contact with solid bodies, which is owing to the motion of their parts: as this property would make a difference between the states of a substance much greater than there is occasion to suppose in the explanation of the other phenomena; as, besides, this celebrated philosopher has fixed the attention on an object which had been neglected, and has drawn applications from it, beneficial in the arts, and in the uses of life, I think it proper to offer some doubts on the principles which he has deduced from his observations: I shall, in the first place, examine whether the facts on which he relies, cannot admit of a natural explanation from the properties which I have already analysed, or whether it will be necessary to have recourse to particular properties. But I shall attend only to the considerations which may serve to elucidate this discussion, without introducing the details it would require, if I were to examine it more fully.

Detail of Count Rumford's experiments on heated fluids.

The experiments which the author made were performed with an apparatus, of which it will be proper to insert a description. "He employed a cylindrical glass jar of 4.7 inches in diameter, and 13.8 in height; he put a known quantity of water (about two pounds) into the jar, which was intended to form a cake of ice at the bottom of the vessel. For this purpose, the jar with the water was put into a frigorific mixture of salt and ice, the action of which was not long in converting the water into a solid disk adhering to the bottom and sides of the jar; the jar was then removed, and plunged into a mixture of ice and water, to the level of the interior cake, which gave it the temperature of melting ice, or of the zero of the common thermometer. Then, after having covered the surface of the ice with a disk of paper,† hot water was poured

* From his *Essai de Statique Chimique*.

† *Bibl. Brit.*

on it, as gently as possible, to about the quantity of 74 ounces; this water was about eight inches above the surface of the disk." Detail of Count Rumford's experiments on heated fluids.

"The paper was then removed very gently, and after having suffered the water to remain a certain number of minutes in contact with the ice, it was poured off, and the jar with the ice which it still contained immediately weighed; its difference from the primitive weight established the quantity of ice which had melted while the hot water remained above it."

Having observed that the motion occasioned by pouring on the hot water produced an effect which was considerable, and foreign to the communication of heat, the author successively devised several modes of diminishing it. "He introduced the hot water through a wooden tube, closed at the bottom and pierced laterally with several small holes, through which the water issued upon a wooden disk, also pierced like a sieve, and floating on the water as it rose in the vessel. This disk was removed as soon as the water was poured in, and the vessel was covered with a wooden lid, in the centre of which was suspended a thermometer; finally, by previously covering the ice with a stratum of cold water, about half an inch in thickness, in which the perforated wooden disk floated; the author succeeded in greatly diminishing the irregularity of the results."

Besides, these precautions, the author separated from his results the quantity of ice, which liquefied at the first instant, and which exceeded that which melted in the succeeding spaces of time: in these different experiments, while that part of the cylinder which contained the ice was kept constantly at the temperature of melting ice, the upper part was left in contact with the surrounding air, or surrounded with a bad conducting substance, or plunged into the mixture of water and ice: the water poured on the ice received different temperatures. I make three divisions of the results of all the experiments: 1st. Water which was only about four degrees above zero, melted a little more ice in the same space of time than boiling water; 2d. When the upper part of the cylinder was wrapped in a bad conducting substance, the hot water melted more ice than when it was in contact with the air: 3d. When the upper part of the cylinder was plunged into the mixture of ice and water, more ice was melted than when it was left in contact with the atmosphere at 61° Fahrenheit's thermometer.

To

Explanation.

1. More heat will be communicated the greater the difference of temperature.

2. Locomotion increases this effect in fluids, and ought to be separately considered. 3. The locomotion does not follow the difference of temperature.

To explain these observations, the properties which we have recognized in liquid substances, and in elastic fluids, and from which we have inferred the changes which are effected in their different states of combinations, must be applied to the phenomena observed by Rumford.

We have seen, 1st. that the liquid particles enter so much the more rapidly into combination as they were at a greater distance from saturation, because then the force which solicits the saturation is greatest; so that the effects which depend on the communication of the temperature must be very weak, when the differences between them are but small.

2d. Locomotion, which serves to bring particles together which are at a greater distance from saturation, accelerates the effect of the mutual action by which its equilibrium is established, so that it is necessary to separate the effect which depends on this cause from that which is owing to immediate communication.

3d. Water and some other substances acquire a greater specific levity on approaching the term of congelation; whence it follows that the locomotion produced by the variations of temperature in other circumstances will be subject to modifications, which must be allowed for when water and the other liquids which possess this property, approach the term of congelation.

To apply these properties, we must also take into consideration the direction in which the heat is communicated; for the combination of effects will be different accordingly as it is applied to the inferior or superior part of a liquid.

Locomotion is greatest when the difference of temperature is small.

In order that a ready motion may be established between the particles which are at the bottom of the vessel, and those at the surface, there must be but little difference between their temperature; the particles which are near the ice, and become expanded, will then raise themselves above those which have a temperature barely greater; but if the temperature should cause a great difference between the specific gravities, this motion will be much more confined, so that the ice will remain surrounded with water of its own temperature, or which is very little removed from it. It is evident, therefore, that that part of the effect which depends on the motion will be much less, when there is a great difference in the temperatures.

The effect of the heat by a fluid will be

But when this distance in fact exists, the effect produced by the communication of the heat, independently of the motion,

will vary according to the manner in which the temperature is preserved in the liquids. If the vessel has a non-conducting covering, the heat will be retained, and a greater quantity will be communicated than if it had been allowed to pass into surrounding bodies.—But when the difference of the temperature of the liquid is not considerable, as in the experiment in which water at 16° was employed, it is more advantageous to augment the effect owing to the translation of the particles, by cooling all the cylinder, than to preserve that which is owing to the simple communication of caloric. It appears to me that this explanation naturally flows from the known properties of fluids, and that Rumford's observations do not lead us to new inductions

greatest where the difference of temperature is most favourable to both the causes of its communication. C. Rumford's experiments require no new law to explain them.

It must be remarked that by separating the effect which took place at the first, when a considerable difference in the temperature could occasion a quick communication, he only observed that which was produced when there were but very slight differences between successive strata of the liquid and the ice itself: now, when there is but a small difference of saturation, either between chemical combinations, or between the temperatures, the equilibrium is established very slowly, and it becomes difficult to appreciate the effects.

Additional remark.

The experiments which Rumford made by plunging a small cylinder of iron, heated to the degree of the ebullition of water, into water and mercury standing over a small piece of ice, without producing its liquefaction, only prove that when two bodies differ but little in their temperature, the equilibrium is established with difficulty; for it must be observed that the iron, which had but a little specific heat, and is a good conductor, must have lost the greatest part of its heat rapidly, in that part of the liquid which it passed gently through, and nevertheless have raised that of the liquid but little, or even that of the mercury, considering the mass of it.

Experiments with mercury and heated iron.

But in these experiments of Rumford I find proofs of the property which he denies to liquids.

Other facts to shew that fluids are proper conductors.

1st. In all the experiments which I have quoted, except in those made with the heated cylinder of iron, the liquefaction of the ice took place in a considerable degree, and each part liquified supposes a quantity of heat which would have raised an equal weight of water from the term of congelation to 75 degrees of the centigrade thermometer.

2d. He

2d. He froze the water at the surface of mercury cooled by a frigorific mixture; the temperature of the mercury was therefore communicated to the water, and the latter yielded its caloric to the mercury, to replace that which it lost.

If the communication of heat was only the effect of the particles of a liquid, the mercury of a thermometer would scarcely change its temperature when it had arrived at the freezing point of water: in fact in several of his experiments (*Essay* 7.) Rumford supposes, that at this degree, the mercury no longer communicates heat: now a thermometer takes the temperature of neighbouring bodies very rapidly, and indicates it several degrees below the freezing point of water, and as far as its own congelation; then it conducts itself like the solid bodies, and its dilatations become proportionably smaller than the preceding.

Mercury conducts better than water, but being denser, has less locomotion, ergo, &c.

Rumford has proved that the conducting power of mercury is to that of water as 1000 to 313.

This effect of the mercury, which takes the temperature of the system in which it is placed more rapidly than the water, although it has a much greater specific gravity, and is much less dilatable by the same degrees of heat, and consequently the heat will cause much less locomotion in its particles than in those of water; this effect I say, proves that the changes of temperature do not depend on the immediate communication and the changes of specific gravity which produces the approximation of the particles of unequal temperatures, but also on the better or worse conducting property of each substance.

Rumford neglects the radiant heat.

3d. Rumford paid no attention to the radiant caloric, nor did he make any allowance for it; nevertheless the communication of heat established by its means between solid bodies and liquids, through the gases, cannot be doubted, and it may be remarked that when he brought a heated bullet near to ice and tallow, a communication of heat took place which melted the surface of both, without it being possible to attribute this communication to a circulation such as he thinks is necessary.

Experiments of other philosophers.

The ingenious experiments of Rumford have employed the talents of several philosophers, who have already proved that the principles to which they led were not conformable to the true results of observation.

Nicholson found heat to pass downwards through a fluid.

Nicholson, in conjunction with Pictet, made some experiments by which he proved, that, on heating a liquid at the surface, by the superposition of a body, the heat penetrated, and

and raised a thermometer placed at the bottom of the liquid : to avoid communication by the sides of the vessel, a bad conducting substance was made choice of, and he ascertained, by means of a thermometer placed in the same liquid near the side of the vessel, that no current was established which differed in the temperature : finally, the motion of the bubbles which were disengaged, and the other appearances of the liquid, convinced him that currents were not formed.

In these experiments * it was proved, that liquids were —and that oil conducted much worse than mercury. different in their conducting faculty ; the penetration of the heat from the top to the bottom, was five times slower in oil than in mercury.

Rumford supposed that the slightest changes of specific gravity were accompanied by a locomotion, which produced a current, and he endeavoured to render it visible, by exposing an alkaline liquor, in which were suspended very small fragments of amber, which he found had the same specific gravity as the liquid, to a change of temperature : but Thomson has shown †, that the motions observed in these molecular Thomson found that the motions of solids do not prove any currents in the fluids, were illusory, and that, in these variations of temperature, which are gradual, they appear to be owing only to the difference of specific gravity which they acquire, and to the adherence of air-bubbles, so that some of these molecular move in contrary directions, and run against each other without following the direction of the currents, he has also shown that these floating corpuscles might receive different motions while the strata of the liquid maintained a perfect tranquillity : he put water, tinged blue by juice of red cabbage into a glass vessel ; he afterwards poured clear water on it with great precaution, by means of a tube with a capillary extremity ; thus he kept the two liquids separate and distinct ; he then heated the vessel gently at the bottom : it is manifest that if a current had been established, it would have been marked by the coloured liquid, but the separation of the two liquids was preserved unconfused ; moreover the corpuscles put into the first liquid moved upwards and downwards, and crossed the line of sepa- —the floating solids rose and fell thro' two quiescent fluids of different colours.

* Bibl. Brit. Tom. XVIII. or Philosophic Journal, Quarto series. V. 197.

† Nicholson's Journal, Octavo, for Feb. 1802. See also a memoir by this philosopher, in the Journal for March, 1801, containing the earliest experimental examination of the Count's doctrine.

ration without producing the mixture of the two fluids, so that their various motions were not the effect of a current which carried them with it, and nevertheless, the heat was communicated to all the liquid. The propagation of the heat, and the agitation of corpuscles, which have nearly the same specific gravity, may therefore take place, independently of the circulatory motion, which is only established when there is a difference of temperature of a certain intensity between the different strata of a fluid.

Murray made experiments in a vessel of ice, containing a fluid. The heat passed downwards,

Murray has opposed Rumford's opinion with experiments still more direct, and not less conclusive; * he placed the bulb of a thermometer in a cylinder of ice, which he filled alternately with oil and mercury; he afterwards brought a heated body near the surface of the liquid; the thermometer rose several degrees in both experiments; but the heat could not have been conveyed by the sides of the ice whose surface would have absorbed and liquefied; no current was established, for the molecules of the liquid having become lighter, could not take a contrary direction, and the author avoided using water, which contracts on passing from the degree of congelation to a temperature a little more raised: the heat must therefore have been communicated to the bulb of the thermometer without the establishment of such a current as is supposed to be requisite; and that which served to dilate it was only the excess of what had liquefied part of the ice.

—and mercury conducted better than oil.

The observations of Murray prove at the same time that mercury is a much more effective conductor of heat than oil, for the elevation of the thermometer was manifested by its intermedium in a much shorter time, and more ice was liquefied.

* Ann. de Chim. Floreal, An. X. or Philos. Journal, Octavo, I. 165. 241. The experiments of Thomson and Murray, originally appeared in our Journal, and those of Count Rumford are also given, for which see the Indexes.

(To be continued.)

XXI.

On the Difficulty of obtaining Alumine in a State of Purity.

By R. T.

To Mr. NICHOLSON.

SIR,

YOUR Journal being open to every disquisition which may contribute to the progress of science, it may not be deemed impertinent to ask you, or some of your correspondents, to point out the best method of preparing pure alumine.

You may, probably, refer me to our modern authors on chemistry, but I aver that the methods therein recommended do not answer the purpose. For if a saturated solution of alum of commerce, be decomposed by a like saturated solution of a carbonated alkali, the alumine obtained is harsh to the touch, rather spongy, and strongly adheres to the tongue.

This earth, although washed as often as you please, always reddens the blue juice of the flowers of mallow, as well as of other delicate vegetable blues. It may be wholly dissolved in about 100 parts of boiling water: and the solution becomes very turbid by muriate of barytes.

If, on the other hand, a dilute solution of alum be decomposed by another of alkali, a quite different product will be obtained. The alumine produced, on being desiccated is not porous, but splits into pieces like starch; and has, before it is nearly dry, a certain degree of transparency; it breaks with a smooth and nearly conchoidal fracture; it does not adhere to the tongue like the former, and has no earthy appearance. This, like the former, cannot be freed from the adhering acid. It also changes fine vegetable blues to red, although ever so much washed. What is the reason of this? Is there no method of forcing this earth from the adhering acid? or is it perhaps a characteristic of the earth itself, to redden vegetable blues. If you will please to answer this question, or point out a better method for procuring this earth pure, you will much oblige,

Camden-Town,
May 28, 1804.

SIR, Your constant reader,
R. T.

REPLY.

THIS letter having arrived so late in the month, I can only for the present offer it to my other correspondents.

SCIENTIFIC

SCIENTIFIC NEWS.

New Earth.

New earth.

A NEW earth has been discovered by Professor Klaproth of Berlin, in an ore which was hitherto supposed to contain tungsten, to which he has given the name of ochroit earth, the mineral which contains it he has called ochroit, (ochroites.) This earth seems to form the connecting link between the earths and the metallic oxides. Like yttria, it produces a reddish coloured salt with sulphuric acid; and is precipitable by all the prussates; but it is distinguished from yttria by not forming sweet salts, and by not being (at least much less) soluble in carbonate of ammonia, and by acquiring when ignited, a light brown colour. This earth farther differs from yttria, by not being soluble either by borax nor by phosphates, with which yttria fuses into a colourless pellucid bead. Its other characteristics, and method of obtaining will be given in our next Journal.

Suberic Acid from Paper.*

BRUGNATELLI has observed, that when nitric acid is made to act upon paper, a large quantity of suberic acid, mixt with oxalic acid, is obtained. This proves that Fourcroy was right, in placing cork among the immediate principles of vegetables.

Easy and expeditious Method of preparing Copal Varnish†.

DEMMENIE, an ingenious glass blower, has noticed that the solution of copal may easily be effected, by exposing it to the vapours of alcohol or oil of turpentine. For that purpose an alembic may be filled $\frac{1}{2}$ with either of these fluids, and some pieces of copal suffered to be suspended by threads in it, over the surface of the fluid. After having made the alcohol or oil of turpentine to boil, the copal becomes liquefied, drops into the fluid and becomes dissolved. When no farther

* Gehlen's *Journal of Chemistry*: Vol. I. p. 3. page 340.

† Ibidem.

solution

solution takes place, the whole is suffered to cool, and the solution of copal is decanted from the undissolved part. The varnish has no more colour than the copal itself.

*Large Piece of Amber *.*

A PIECE of amber weighing 13lb. 7 oz. 9 scr. and measuring 318½ cubic inches, has lately been found at Schlapacken, near Gumbinnen and Insterburg in Germany, which is the largest mass of amber hitherto found. Its colour is a pale yellow, intersected with several lines. Its value is estimated at about 40,000 dollars. Large specimen of amber.

Fluoric Ether †.

15oz. Previously ignited and pulverized fluuate of lime, were introduced into a retort containing 10oz. of highly rectified alcohol, and an equal quantity of sulphuric acid of 1,860 spec. grav. and the mixture distilled to dryness. During the distillation, a large quantity of gas was evolved, which burnt with a beautiful blue flame, and diffused an odour resembling phosphorated hydrogen. During the combustion of this gas, vapours of fluoric acid were precipitated. The product which had been obtained during the distillation was again distilled to one half, and the product which passed over, was poured into a vial containing water. No heat was produced, nor did the two fluids mix. It was therefore ether. But as its taste was sour, I added to it a solution of potash; this instantly separated a considerable quantity of siliceous matter, and the whole became converted into a gelatinous mass. The whole mass, on being again distilled, yielded pure fluoric ether. It greatly resembled sulphuric ether; its specific gravity was 0,720; it burnt with a blue flame; its taste was bitter, and greatly resembling bitter almonds.

New Method of preparing Nitric Ether. By BRUGNATELLI ‡. Nitric ether.

INTRODUCE into a tubulated retort, one ounce of sugar, and pour over it two ounces of highly concentrated alcohol. Adapt to the retort a capacious receiver surrounded with

* Ibid.

† Ibid.

‡ Ibid.

cloth

cloth dipt into water; and secure the junctures of the vessels by surrounding them with slips of paper only. Having done this pour through the tubulure of the retort 3oz. of concentrated nitrous acid; a violent action takes place, the sugar becomes dissolved, and the alcohol converted into ether, passes over into the receiver; its quantity is nearly equal to the alcohol employed.

*Ascension of Sulphuretted Hydrogen Gas, by the Affusion of Nitrous Acid. By Professor LICHTENBERG *.*

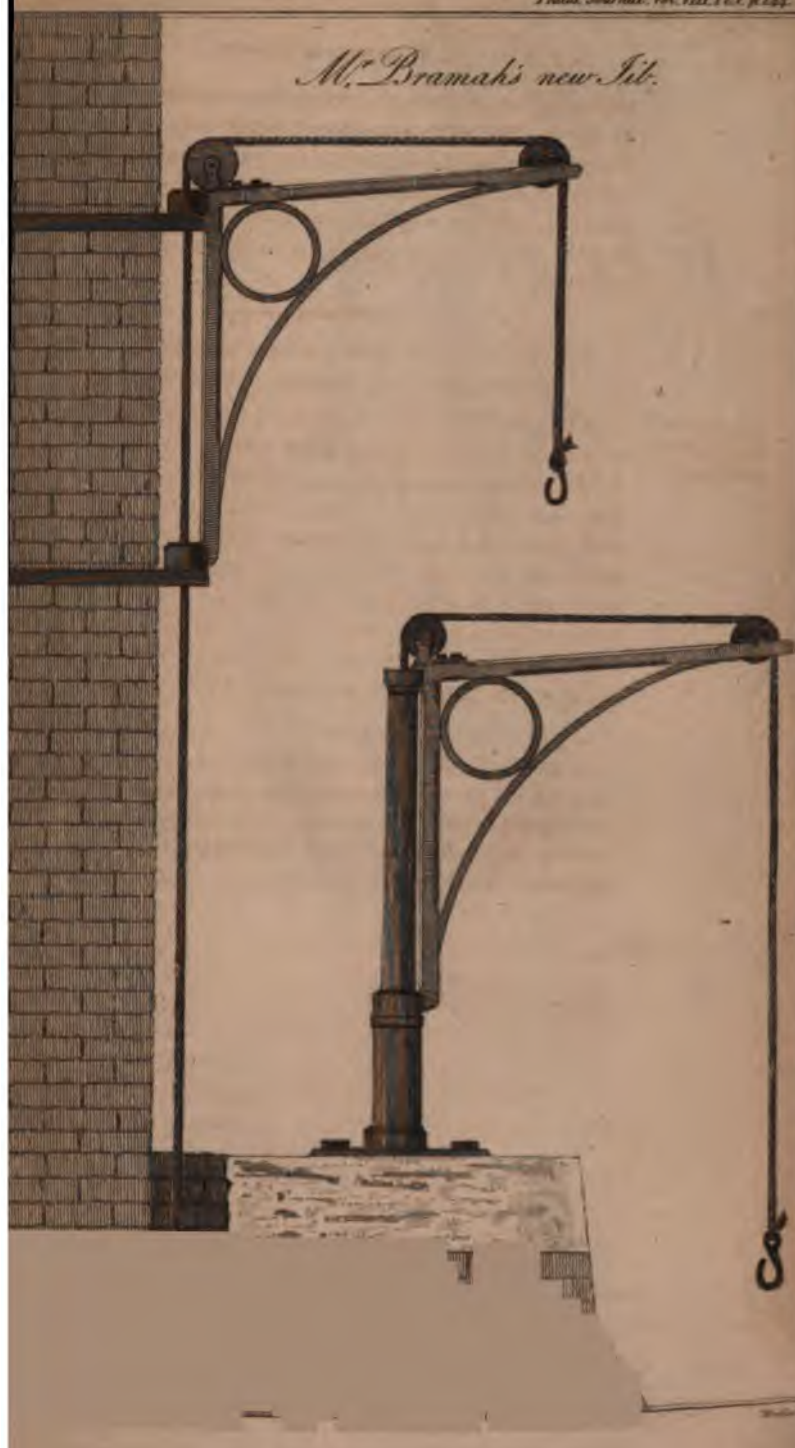
Ascension of
sulphurated hy-
drogen gas.

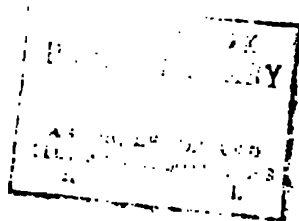
ATTEMPTING to illustrate the decomposition of sulphuretted hydrogen gas, I filled a bottle with it, capable of holding about 18oz. of water. Having done this, I poured into it at once, $\frac{1}{4}$ of an ounce of nitrous acid; a hissing noise took place, and much red vapour was disengaged, which in order not to molest my auditors, I confined in the bottle, by corking it. No sooner had this been accomplished, the mixture exploded with a loud report, accompanied with a blue flame†. The pieces of glass were thrown to a considerable distance, the larger ones were covered with sulphur.

* Ibid.

† For the success of this experiment, it seems to me to be necessary that the gas must be obtained by decomposing water by means of sulphuret of iron and an acid; for it always failed in my hands, if an earthy or alkaline sulphuret had been made use of for the production of it. F. A.

Mr. Bramah's new Lift.

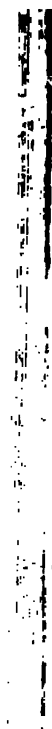




Filtering Apparatus.



Galvanic Apparatus



*Ancient wooden Lock,
used in Egypt, and in most parts
of the Turkish Empire.*





Ancient Egyptian Lock, developed.

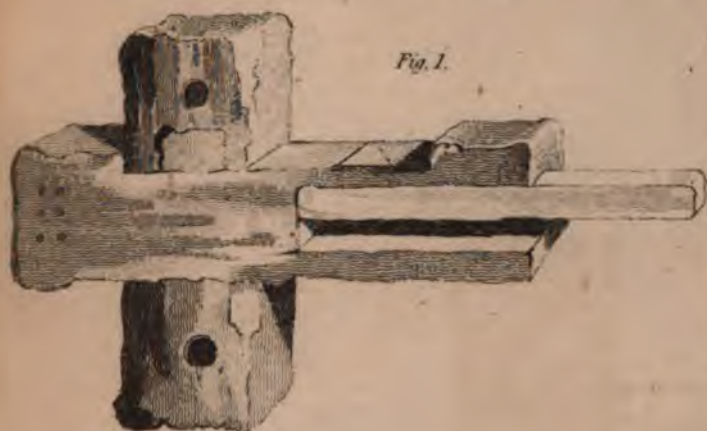


Fig. 1.



Fig. 2.



Fig. 3.

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY, 1804.

ARTICLE I.

*On the supposed Chemical Affinity of the Elements of Common Air;
with Remarks on Dr. Thomson's Observations of that Subject.
In a Letter from Mr. J. DALTON.*

To Mr. NICHOLSON.

SIR,

IN a former letter, inserted in your Journal (new series, vol. III. page 267) I endeavoured to shew the absurdity of the notion of atmospherical air being a chemical compound of azotic and oxygenous gases. Besides the difficulty, or rather impossibility on the one hand of conceiving how two elementary particles, constantly repelling each other, should notwithstanding be held together by a principle of cohesion or chemical affinity; or on the other hand, supposing the two atoms to combine, and form one centre of repulsion, how atmospheric air should differ from nitrous gas, &c. There are a variety of facts which oppose the doctrine so forcibly that I have for some time wondered on what grounds those who are still its adherents defended it. Dr. Thomson, in the second edition of his chemistry, vol. III. page 316, after reviewing the opinions of different philosophers on this head, and amongst others my own, concludes that air is a chemical compound; he assigns the four following reasons for the conclusion, which, from his extensive

Difficulties respecting the position that atmospherical air is a chemical compound.

Dr. Thomson's arguments in favour of that doctrine.

VOL. VIII.—JULY, 1804.

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acquaintance with authorities, may fairly, it is presumed, be deemed the most cogent that have been offered on that side of the question. It is the object of this communication to shew their insufficiency.

1. The constant proportion of oxygen and azot supposed to be regulated by affinity, or increased attraction of the abundant gas.

—but this may with more probability be ascribed to diminished repulsion of the deficient gas: For, &c.

2. Humboldt and Morozzo's experiments, that a mere mixture of the gases differs from atm. air:—not credited.

3. Different combustibles absorb different quantities of oxygen from common air.

1. The constant proportion of azot and oxygen in the atmosphere is considered as an argument for their being held by affinity. So indeed it may; but it is equally in favour of my hypothesis, and therefore nothing tending to decide the question can be obtained from it. For, let part of the oxygen be abstracted any where from the atmosphere; then the azot may be supposed to attract the oxygen from the vicinity, and thus the equilibrium be restored: but it is certainly equally satisfactory to suppose that the oxygen in the vicinity, meeting with a less repulsive power from the deficient quarter, nothing prevents its diffusion into that quarter but the azot previously there, which, by hypothesis, can only retard, but by no means prevent the effect. Thus then, whether the azot attract the oxygen, or the oxygen repel itself, the effect is precisely the same. From this fact simply, it is impossible therefore to decide the merits of either theory; but if it be found that any one gas diffuses itself in any other, with nearly the same celerity, it will be a presumption in favour of my hypothesis; if otherwise, it may be urged that the quicker diffusion is owing to the stronger affinity. I have made a great number of experiments on this head, but could not find any remarkable difference in the time and circumstances of diffusion of the same gas.

2. It is said the experiments of Morozzo and Humboldt shew that air possesses different properties from a mere mixture of its two component parts. I do not credit the experiments.—Humboldt finds a variable quantity of oxygen, from 25 to 30, or more per cent. in the air; whereas others who are more accurate, find but 21, or at most 22, and that constant. It is no wonder then, if he mix 28 oxygen and 72 azot, that the mixture diminishes nitrous gas more than air, and supports combustion and animal life for a longer time.

3. Different combustibles are capable of absorbing different portions of oxygen from a given quantity of air. Phosphorus 22 per cent. Sulphur, 8, &c.—The only inferences I draw from these facts are, that phosphorus will burn in oxygen of any density, that sulphur will not burn in oxygen unless it be of $\frac{3}{4}$ of atmospheric density or more. The difference in the phenomena

mena of combustion in air, and in oxygen is not to be ascribed to the combination of azot and oxygen, but to the less density of the latter, ($\frac{1}{3}$) of what a pure atmosphere of the same gas would be. From an incidental but imperfect trial I made, in conjunction with Mr. Davy last winter, I have no doubt but iron wire would burn in common air of five times the density with brilliancy as in an atmosphere of pure oxygen of common density. At any rate it is notorious that as the density of common air is increased, combustion in it becomes more vigorous. Though I have never attempted combustion in an atmosphere of pure oxygen of $\frac{1}{3}$ the common density, I can scarcely doubt that the appearances would be much the same as in the open air. It is probable therefore that the facts under this head, if duly investigated, would turn out in favour of the hypothesis of air being a mixture.

Ascribed merely to the various densities of oxygen in which they cease to burn.

Proposed experiments to decide this point.

4. "A gas no way distinguishable from common air frequently makes its appearance during the preparation of nitric acid; and Mr. Davy decomposed nitrous oxide, by passing it through a red hot tube, and converted it into nitric acid and a gas, which possessed the properties of common air; now if air were a mere mixture, it is infinitely improbable that its two constituent parts should be evolved during such processes exactly in the proportion that exists in common air."—Granted; but as the force of this argument rests upon the *exact proportion* of oxygen and azot in the gases so evolved, that is, upon their being constituted always of 21 per cent. oxygen, and 79 azot, the facts should be made out accordingly. Dr. Priestley is the only one I know of, who has particularly examined the gas produced in the preparation of nitric acid, and he found it to have *much more* oxygen than common air. Mr. Davy in his analysis of nitrous oxide, found the gas analogous to atmospheric air always to contain *less* oxygen, though it was nearly of the atmospheric standard.

4. Gas resembling common air, obtained in preparing nitrous acid.

Answer. The precise resemblance, or proportion of parts, has not been shewn.

The quick ascent of hydrogen and the descent of carbonic acid, have been objected to my hypothesis as facts that prove the operation of the laws of specific gravities on elastic fluids. No doubt can exist that a portion of elastic fluid completely insulated, as a balloon, or a bubble of carbonic acid or hydrogen, surrounded by a film of water, is subject to the laws of gravitation, and rises or falls in elastic fluids on the same principle.

The statical ascent or descent of the gases affords no proof against the law of their general diffusion.

For the separate masses are carried by their relative gravity more speedily than the diffusion can take place.

But the diffusion is much speedier than is generally taught.

The facts, if ascribed to affinity, would shew that it is the same between all the gases—and even between a gas and a vacuum.

ciple as it rises in water; the same must be allowed when a vessel, containing a considerable portion of elastic fluid, is suddenly exposed at some surface to the atmosphere; in this case, the fluids *must* operate upon each other for a few moments in a collected capacity, as in elastic bodies; because the diffusive or repulsive force by which they constantly tend to dispersion, is comparatively slow in producing the ultimate effect, being in this respect exactly similar to chemical affinity, the operation gradually diminishing as the effect draws towards a conclusion. Nothing more therefore can be inferred from the facts above-mentioned, than that gravity overpowers, and for a moment obliterates the effect of that cause which in other cases slowly produces the dispersion of the fluid, whether it be attraction, as commonly supposed, or repulsion, as I suppose. Chemical philosophers have not enquired sufficiently into the effects of exposing gases in different circumstances to the atmosphere; all that we are usually told is, that a jar filled with hydrogen and uncovered, loses its gas in a few seconds; but if inverted, it remains nearly pure for a considerable time, &c. I find that a cylindric jar of 7 inches depth and $2\frac{1}{4}$ diameter, being filled with hydrogen, and inverted, loses more than half of its gas in two minutes, and there is so little left as scarcely to explode in five minutes. If a tube, 12 inches long and $\frac{1}{2}$ inch diameter, be filled with hydrogen, and exposed in like manner to the atmosphere, it will lose half its gas in five minutes, and that the same, whether it be held up or down or horizontal. Here we see effects that cannot be accounted for by gravity, that are produced in opposition to its agency, and where indeed it is almost obliterated by the action of some more powerful cause. Let the advocates for the atmosphere being a chemical compound attend to such facts as these, and they will soon find themselves reduced to acknowledge that *all gases have the same affinity for one another*, a position which their doctrine ultimately tends to establish. Indeed it is the same with regard to air and vapour, of water, ether, or of any other fluid; that is, all kinds of gas or mixtures of gases, have the same affinity for the same vapour, and even a torricellian vacuum possesses just the same affinity as any of them, judging from the quantity evaporated, and force of the vapour in a given volume. If any one doubt it, he may easily satisfy himself by throwing up a drop

a drop or two of ether into the vacuum of a common barometer; if the temperature be 68° , the mercury will fall 15 inches nearly; at the same time, if ether be admitted to a given bulk of any kind of gas, subject to the pressure of the atmosphere, the volume will be doubled, clearly shewing that the elastic vapour from the ether is the same in both cases, namely, an independent fluid of 15 inches force.

I cannot dismiss this subject without observing, in justice to Dr. Thomson, that he has entered more clearly into my views of these subjects than any other of our own country who has animadverted upon them. There are certain principles, however, which he, with most chemists of the present day, embraces, which are, according to my experience, decidedly erroneous. One of these is, *that water dissolves air*. An excellent paper of Mr. W. Henry, on the absorption of gases by water, in the *Philos. Transactions* for 1803, has shewn us sufficiently in what light we should view the supposed solution of air in water. Certainly air that is retained in water by mechanical force, and which always escapes when that force is withdrawn, cannot with any propriety be said to be held by chemical affinity.

Water does not dissolve air.

Dr. Thomson has been misinformed respecting my opinions on the expansion of liquids. In vol. i. page 343, he gives it as my suggestion, that all liquids expand the same quantity from their freezing to their boiling temperatures. I never entertained such an opinion; and it is certainly erroneous. My idea is, that pure and homogeneous liquids, such as water and mercury, expand according to the square of the temperatures from the points at which they congeal; but I have not yet found a law to regulate the *relative* expansions of these and other liquids.

The author does not affirm that the expansion of all liquids by heat is the same between congelation and boiling; but that any single fluid expands according to the square of the temperature from its freezing point.

I am your's, &c.

J. DALTON.

Manchester,
June 16, 1804.

Easy



*Ancient wooden Lock,
used in Egypt, and in most parts
of the Turkish Empire.*





Ancient Egyptian Lock, developed.

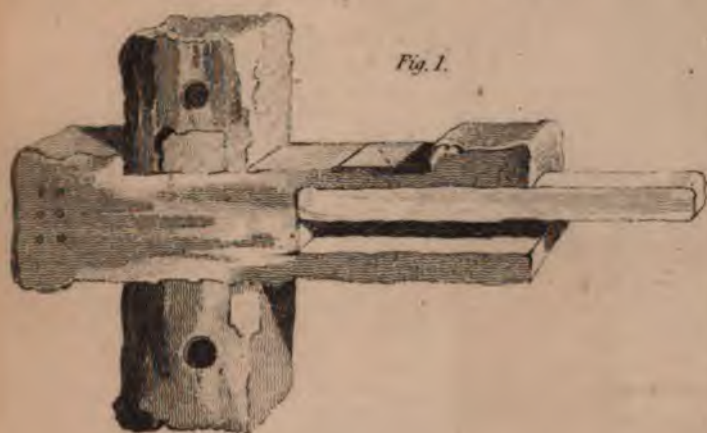


Fig. 1.



Fig. 2.

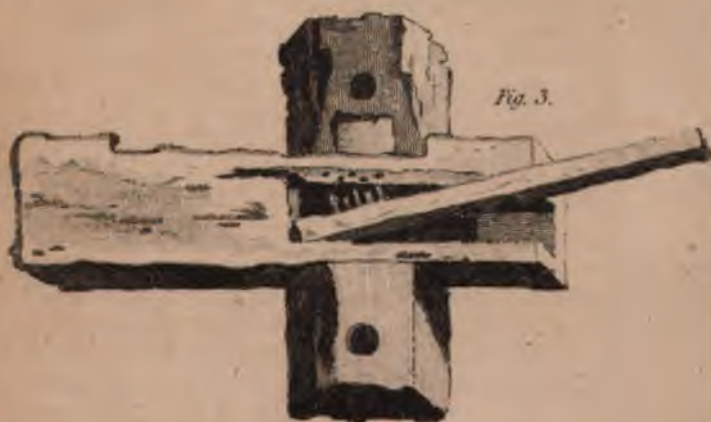


Fig. 3.

ably simplified. For then $M = 2 \sin \alpha'^2 \cos. \lambda \times \frac{\phi - \lambda}{\omega}$, and $N = M (2 \sin \lambda^2 - \frac{1}{2}) + \cotang. \alpha' M^2$; and if $\alpha = 90^\circ$ seré, M is $= 2 \cos. \lambda + \frac{\phi - \lambda}{\omega}$, and $N = M (2 \sin \lambda^2 - \frac{1}{2})$.

The particular states of the data, when the term involving the second power of δ is rigorously equal to nothing, may be thus determined:

In the first place, if $\delta = 0$, or the eccentricity of the spheroid be infinitely small, α , α' , and β , β' , are exactly equal to each other.

Secondly, when $M = 0$, N is $= 0$, but M is $= 0$ when $\phi = \lambda$, or the line $\alpha' = 0$, that is when the triangle is isosceles, or the directions of the places due north or south of each other.

It is almost rigorously exact.

It appears then that this property of spheroidal triangles, first advanced by Mr. Dalby, and objected to by Mr. Playfair, is almost rigorously exact; and it might easily be shewn, that its application will never occasion any material error, even in the most unfavourable case that can be proposed. And it is not merely an elegant and curious theorem, but is highly valuable, as affording a method of determining the longitudes of places from terrestrial measurements, almost independent of all hypothesis. For whether the earth be an exact ellipsoid or not, any small portion of its surface may certainly, without error, be considered as pertaining to one of small eccentricity, which supposition is all that is necessary for demonstrating the theorem.

Determination of the eccentricity at place of obs.

Our solution also affords an easy method of determining the eccentricity at the place of observation. For if we have the latitudes and difference of longitude given, we shall also have the horizontal angles on the sphere. But from observations of the pole star, we may find the horizontal angles on the spheroid, and consequently the difference between them; but this difference is equal to a certain function of δ in our solution, whence we shall have an equation, from which δ may be determined. Thus if α be the observed horizontal angle on the spheroid, and α' the computed one on the sphere, we have $M \delta + N \delta^2 = \alpha - \alpha'$. Now if we reject the term $N \delta^2$ as insensible, we obtain a near value of $\delta = \frac{\alpha - \alpha'}{M}$, which substituted for δ in

$M + \lambda$, gives $\lambda = \frac{\alpha - \alpha'}{M + \frac{\alpha - \alpha'}{M}}$, very nearly. Here a question

naturally arises; viz. To determine the value of α' such that λ may be obtained in this manner with the highest degree of accuracy, of which the method is susceptible.

This will evidently be the case when M is about its maximum value. So that if we put the differential of $M = 0$, and substitute the value of the differential of α' resulting from the properties of the spherical triangle in this equation, we shall have the required value of the horizontal angle. Let us suppose λ and

ω given, and there will result $\sin \alpha'^2 \times \frac{\sin \phi - \sin \lambda}{\cos \phi}$ a maxi-

mum; whence $2 \cotang. \alpha' \cos \phi (\sin \phi - \sin \lambda) d\alpha' + (1 - \sin \lambda \sin \phi) \times d\phi = 0$. But by spherics,

$$\cotang. \alpha' = \frac{\cos \lambda \tan \phi - \sin \lambda \cos \omega}{\sin \omega},$$

and $d\alpha' = -\frac{\sin \alpha'^2 \cos \lambda}{\cos \phi^2 \sin \omega} \times d\phi$; wherefore by substitution,

$$2 \sin \alpha' \cos \alpha' = \frac{\cos \phi (1 - \sin \lambda \sin \phi)}{\cos \lambda} \times \frac{\sin \omega}{\sin \phi - \sin \lambda}.$$

Also when ω and $\phi - \lambda$ are small, the $\sin \omega = \frac{\tan \alpha'}{\cos \phi}$.

$\left\{ \sin (\phi - \lambda) + 2 \sin \lambda \times \cos \phi \sin \frac{1}{2} \omega^2 \right\}$, and $\sin \phi - \sin \lambda$

$= 2 \cos \phi \frac{\phi + \lambda}{2} \times \sin \frac{\phi - \lambda}{2}$; consequently $2 \cos \alpha'^2 =$

$$\frac{(1 - \sin \phi \sin \lambda)}{2 \cos \lambda \cos \left(\frac{\lambda + \phi}{2} \right)} \times \frac{\sin (\phi - \lambda) + 2 \sin \lambda \cos \phi \sin \frac{1}{2} \omega^2}{2 \sin \left(\frac{\phi - \lambda}{2} \right)},$$

and $2 \cos \alpha'^2 = 1$ if we suppose $\phi = \lambda$ nearly, and $\sin \frac{1}{2} \omega^2$ very small.

When the horizontal angle α' , therefore, is equal to 45° or 135° , the observations will be nearly in their most favourable state for determining the compression by means of our theorem.

We might illustrate this method by examples taken from the trigonometrical survey of Great Britain, but on reference to it I have found fewer complete sets of observations than might be expected, and such as are complete in every respect, are not well calculated for this purpose, the horizontal angles being

ing nearly right ones. The observations at Beachy Head and Dunnoose give $\frac{1}{125,1}$ for the compression; but it must be remarked, that the state of the data is very unfavourable in this example.

The rules which our solution gives for computing the horizontal angles from the latitudes and difference of longitude, will be found, I apprehend, much shorter than Mr. Dalby's, besides the advantage they possess of affording us the means of ascertaining the figure of the earth by a very simple process, from observations made with the same instruments and by the same observers.

The preceding rules applicable to an ellipsoid; but the managers of the trigonometrical survey avoid this assumption.

The theorems we have been detailing, with some others which may perhaps form the subject of another letter, would give us the relative position of one place to another on the surface of the earth, were its figure an ellipsoid of known dimensions; but as this is still considered as problematical, the method adopted by the gentlemen who have so ably conducted the survey of our island, is certainly preferable. They first obtain the length of a degree upon the meridian, and its perpendicular in a given latitude, and employ these data for computing the geographical situations of all the places near that parallel, and not far distant from a known meridian. In the smaller triangles the truth may be thus obtained to the fraction of a second, and in the larger ones they have very successfully employed the beautiful property of spheroidal triangles, which we have so often mentioned.

How to apply the rules.

But though we give the preference to their method of computation, I conceive the preceding rules will be found equally accurate, if we make use of the values of c , a , and b deduced from their observations; or if we assume near values of them, and note the agreement or disagreement of the computations with observations made at a place considerably distant from the first station. We may thus ascertain nearly the error of our suppositions, and then correct the intermediate stations. This cautious method of proceeding is rendered necessary by the anomalies which have been discovered in the measures of degrees in different latitudes, as well as by the general rule, which ought to be our guide in all philosophical inquiries, to frame as few hypotheses as possible, but to make accurate experiments, and infer the truth from them by fair and genuine induction.

I mean

I mean not, however, to support the opinion, that the earth is not an ellipsoid; but, on the contrary, should be very loth to be obliged to give up an hypothesis, which is so beautiful in theory, and has stood its ground so long. Many of our objections to it may very probably arise from errors in observations, or from other causes which have not yet been fully examined. The remarks of Mr. Playfair on this subject are very ingenious, and I hope will be confirmed by the phenomena: but if not, I am convinced we have not the plea of inaccuracy to set up in this instance. One of the strongest objections, however, has been lately done away. The degree of the earth measured in Lapland in the year 1736, has been found, by some Swedish gentlemen sent there for that purpose, to err in excess by no less than 203 toises. Now if we advert to the number and character of the astronomers who originally measured this degree, it will be difficult for us to set limits to the errors of other observers. Perhaps the anomaly in the degree at the Cape of Good Hope arises from the same cause.

The earth probably an ellipsoid;

particularly as the error of the Lapland degree removes much of the doubts entertained on this subject.

Fortunately, however, the great improvements, which have since been made in the instruments for astronomical observations and geodetical mensuration, afford us the means of bringing the probable errors of observation within very narrow limits. We may thus obtain a number of measures in different latitudes of equal accuracy, and by comparing them together, the question about the earth's figure may be put beyond a doubt. If this comparison shall be found to give different ellipses, we shall then be fully warranted in rejecting the hypothesis entirely and for ever. But till this is done, we may be allowed to adopt an hypothesis, which is so simple, so good in theory, and supported by so many strong arguments and accurate observations.

Late improvements will clear up this matter.

We have already remarked, that the degree lately measured in the Myfore, compared with that in France and England, gives $\frac{1}{100}$ for the compression at the poles: the corrected degree in Lapland gives $\frac{1}{117}$, and that measured in Peru, $\frac{1}{112}$. There is a considerable difference between the compressions deduced from other measures, but the mean falls between these limits. From the best observations of the length of the pendulum that swings seconds in different latitudes, the same conclusion is also drawn; the second pendulum near the pole compared

Mean compression from measurement;

and from the pendulum;

compared with that at the equator, gives $\frac{1}{104}$ for the compression. We may therefore assume $\frac{1}{103}$ as being very nearly its true value.

from celestial irregularities;

s. p. of the moon.

It is a curious circumstance to find the figure of the earth, deduced from the measurement of lines and angles on its surface, confirmed (perhaps corrected), by observations of the stars and planetary bodies in the heavens, combined with the theory of universal gravitation. But such is certainly the case. Among others may be mentioned two small inequalities in the moon's motion, which the industry of modern mathematicians have unfolded. One of them was first taken notice of by Mayer, and fixed by Maſon at $7''.7$, but was neglected by astronomers, as it did not sufficiently appear that such an equation should arise from the theory, till Laplace traced it to the oblateness of the earth's figure. Its argument is the longitude of the moon's node, and its value has been found by Burg, from the observations of Dr. Maskelyne, to be equal to $6''.8$, which answers to a compression of $\frac{1}{303,000}$. There is also another inequality of the moon's motion in latitude, which depends on the sine of the true longitude, and results from a nutation in the lunar orbit, produced by the action of the terrestrial spheroid. Burg has also determined the coefficient of this inequality, from a great number of observations, to be equal to $8''.0$, which results from a compression of $\frac{1}{304,000}$.

Remarks upon Newton's discovery and investigation of this subject.

The precession of the equinoxes, and the nutation of the earth's axis, were discovered by Newton to arise from the oblateness of the earth's figure. This famous problem is acknowledged to be one of the most abstruse in physical astronomy, and its complete solution requires the utmost resources of the modern analysis. The compression thence arising is equal to $\frac{1}{104}$, agreeing exactly with the results from the two lunar inequalities, the lengths of the second pendulum, and the best measurements on the earth's surface. It is well known that Newton failed in attempting to solve this problem, and some French mathematicians have been disposed to pride themselves on being the first to detect it. It ought however to be remembered, for the honour of that great man, that his mistake did not arise from any error in principle, but from his taking for granted, without demonstration, a circumstance which appears highly probable, but is really erroneous. He seems to have

have first made this assumption in order to shorten his solution, and on finding the calculations to agree with observation, to have never after returned to the subject. The mathematicians of his time were unwilling or unable to follow him, and the question remained as it came from his hands, till the middle of the last century. Had doubts arisen and objections been started, the genius of Newton might have been once more roused to action, and continued to enlighten the sciences to the last. But unfortunately no such incentive was given, and Newton stopped short in the career of his glory. The evening of his life was devoted to other studies; but however usefully he may have been employed, there are few who will not be inclined to lament that he ever laboured in the Mint or the Revelations.

Thus much I have thought it not entirely foreign to observe on one of the most remarkable effects of the oblateness of the earth's figure, and in justice to its immortal discoverer, the inventor of the modern analysis, the father of physical astronomy, and the preceptor of Europe.

I am, Sir,

Your most obedient servant,

PEREGRINUS PROTEUS.

Portsmouth,
May 6, 1804:

Corrections to be made in the first Letter.

In page 12, line 20, after *paper* insert *but*. In page 15, line 23, &c. divide the coefficient of the term $\frac{c}{a}$ by $\cos. \lambda \cos. \phi$; also in page 16, lines 3 and 12. In page 16, line 9, *dele* and $\cos. \lambda \cos. \phi$; and also in line 14. In page 17, line 6, &c. for $\cos. \omega$ read $\sin \omega$. In page 18, line 25, for Pc, Qc, Rc , read $P \frac{c}{a}, Q \frac{c}{a}, R \frac{c}{a}$.

IV.

*Description of a striking Part for a Clock, in which the Intervals between Stroke and Stroke are not regulated by a Fly, but by a Pendulum. By Mr. EDWARD MASSEY.**

SIR,

New striking
part for a clock.

HAVING for a number of years considered that a method of striking a clock, at certain regular intervals, might be of great service in making observations on the heavens, and ascertaining the velocity of sound, &c. I beg leave to lay before the Society for the Encouragement of Arts, &c. a striking part of an eight-day clock, which I have no doubt will answer the purpose intended; and if, upon examination, the Society should be of opinion that it may be useful, I trust they will reward it according to its merit. They will find that the work of this model is less than that of the common striking movements, and may be made by a common workman, with less expence and trouble; the weight required is also considerably less. The principle I act upon is the pendulum, by which I regulate the stroke, instead of the fly; the advantage of which must be obvious to every one. The machine consists of a toothed wheel A, one pinion B, a pin wheel C, pallets D D, pendulum E, and locking detent G. The hammer-work F is as usual, and strikes on the bell at H. The weight hangs to the cord I. See Plate XII. Fig. 1 and 2, where a front and side view of the machinery are given, and where similar letters denote the same parts in each view.

Description of the Machinery.

I consider it is only necessary for me to give the description of the wheels, so as to be a direction to a mechanic, who wishes to manufacture clocks on this principle. The main-wheel A, with seventy-eight teeth, is to act in a pinion of eight leaves, B. The pin wheel C should be large, so that the pins on which the pallets D and the locking G act, may be flung as far from the center as possible, which pins may be eight or sixteen in

* From the Transactions of the Society of Arts, 1803. This invention was honoured with a reward of 20 guineas.

number. If eight, the pendulum E should be about nine inches long, and it will vibrate twice betwixt each blow of the hammer; but if sixteen pins are put in the wheel, the pendulum must be about three inches long, and will make four vibrations betwixt each blow. The pins for drawing the hammer must be eight in number, and be fixed in a circle of about half the diameter of the aforesaid pins. The locking-plate is on the main wheel. The stop is against the pins on which the pallets act, and may be discharged by a flint piece.

New striking
part for a clock.

As I have described the model, I beg leave to point out the method of striking a clock by the common pendulum, true seconds, without any additional pendulum or pallets for the striking part.

Description.

Fix a cantrite wheel with sixty teeth on the same arbor with a swing wheel of thirty teeth. Now suppose a striking part to be made in the common way of making an eight-day clock, so far as the pallet pinion, leaving out the warning and fly pinions. A crank piece must be fixed on the pallet pinion, which must come into contact with the cantrite wheel, which is fixed on the swing wheel arbor. Then suppose the clock to be set a going, and the rack discharged, the pallet pinion will make a revolution on every vibration of the pendulum, by which means a clock will strike seconds as true as a pendulum vibrates, which I hope will be considered as useful for the purposes I have described. I also beg leave to observe, that a great advantage arises in both the above machines, from their not being liable to foul, as the stroke is given by the certain and regular vibration, instead of the uncertain motion of the fly. Its advantage likewise depends on the cleanness of the work; and church clocks will be much benefited from the decrease of weight.

I am, Sir,

Your most obedient servant,

EDWARD MASSEY.

Charles Taylor, Esq.

Hanley, in Staffordshire,

Jan. 12, 1803.

V.

Description of a very simple Telegraph, consisting of the Human Figure adapted to converse at a Distance by means of Signs.*

Preliminary observations.

AMONG the great advantages of which those who do not enjoy sight are deprived, we may reckon telegraphy, which it would be difficult to supply by another sense. Such an art for the use of the blind would doubtless be very imperfect. On the contrary, the advantages which it affords to those who enjoy light, appear to me to be so important, that it ought to be rendered more general, and brought within the reach of all men. For this purpose I have offered some notions on the subject. I do not pretend either to be the first, or the only one who has entertained them: I even believe the contrary; and, in this instance, they will have that in common with telegraphy in general, which, though new in its execution, was not so in its invention†.

* From a small pamphlet in French, extracted from *Mémoires sur les Aveugles*, &c.

† It is not extraordinary that the same invention should have been thought of by several persons. Thus the notion of telegraphy may be found in the preface to one of the German works of the celebrated Chr. Louis Hoffman, a native of Rheda, and physician to the Elector of Mentz, which, however, does not lessen the merit of the French inventor. A description of the telegraph invented and executed by Citizen Chappe, is found in the interesting work of Mr. Meyer, intitled, *Fragmente aus Paris, im IV ten Jahr der Französischen Republik*; Hamburg, 1797. The author, who was in the telegraph office at the Louvre with Cit. Chappe, affirms, that the latter had made his discovery before the revolution, that he communicated it to the National Assembly in 1792, and that the Convention, on the report of Lakanal, decreed, July 25, 1793, the establishment of a telegraphic correspondence, under the direction of Citizen Chappe, as telegraphic engineer. A notion may be formed of the rapidity of the telegraphic correspondence, by the following example, of which Mr. Meyer was a witness. He says, that during his presence in the office at the Louvre, and at the appointed hour in the evening, enquiry was made of the office at Lille, by a single signal, if any thing new had happened in the army of the north, and the answer, no, was received in 88 seconds.

Persuaded of the importance of communicating our ideas at distances too great for the voice or hearing, I have employed myself in enquiries for a telegraph which should be at once cheap and sufficiently perfect to be easily used. I believe I have discovered it; nature herself has given it to all the world.

This telegraph is the human body, its branches are the arms, ^{Natural tele-} which, with each other and with the perpendicular line of the ^{graph.} trunk, may form a great number of figures, sufficiently distinct to be readily seen, at considerable distances, by simple vision or by the assistance of a telescope. It would certainly be very agreeable for two friends, living opposite to each other in an extensive place or on the banks of a large river, to be able to converse together. Of what utility might it be to the inhabitants of open countries to have a method of communication which is at once speedy and requires no expence!

I hope, therefore, that the generality of readers will see with pleasure that a method of communication is opened to them, susceptible of being varied and brought to perfection by themselves. In the annexed *Plate X.* are found the signs for all the characters in the alphabet, for the figures, and for the punctuation. To simplify the writing of these signs, the perpendicular and immoveable line, which represents the trunk of the body, may be omitted, as I have done, only indicating it in some signs or characters by a point, as in the *e* and *u*; and to write them with more rapidity, they may perhaps be joined in the manner of short-hand writers.

Three different positions of the right arm, and as many of ^{Telegraphic} the other, form the signs for the vowels. The right arm ^{signs by the hu-} stretched out, and a little raised, forming an angle of about ^{man figure.} 45° with the line of the trunk, gives the sign which expresses *a*; the same arm extended and more elevated, or horizontal, forming a right angle with the trunk, gives *e*; more elevated, and forming an obtuse angle of about 135°, it furnishes *i*. The left arm extended, and forming an angle of about 45° with the trunk, gives us the sign for *o*; more elevated, or horizontal, it signifies *u*; still more elevated, and forming an angle of about 135°, it gives *y*.

It may be observed, were it only to assist the memory, that *a* and *o*, whose sounds have some resemblance in several words, are indicated by the same sign, and likewise *i* and *y*, which in the French language often express but a single sound; with
this

Telegraphic
signs by the hu-
man figure.

this difference, however, that the signs for *a* and *i* are formed by the right arm, and those for *o* and *y* by the left arm. The two other vowels, *e* and *u*, are likewise indicated by the same sign, but with the same difference.

To form *b*, both arms describe an angle of 45° . To form *c* and *d*, the right arm is kept in the same position, and the left arm is raised to the height of 90° for the first, and of 135° for the latter.

To form the letters *f*, *g*, and *h*, the right arm is extended horizontally, and the three different angles of 45° , 90° , and 135° , are formed with the left arm.

To express *j*, *k* or *q*, and *l*, the right arm is raised to the height of 135° , and the same positions are repeated with the left arm as were employed for the six preceding consonants.

To form *m*, the upper part of the right arm is placed in a horizontal line, and the fore-arm is raised at the same time, so as to form a right angle with this part. To designate *n*, the left arm is placed in the same position. To form *p*, both arms describe the preceding figure at the same time.

To express the letters *r*, *s*, *t*, the upper part of the right arm is placed in an horizontal line, and a right angle is formed with the fore-arm, while the extended left arm successively describes the three different angles of 45° , 90° , and 135° .

For *v*, *x*, and *z*, the same is done with the left arm as was done, for the three former letters, by the right, which then forms the three angles of 45° , 90° , and 135° .

If it is intended to shew that a telegraphic signal is terminated, the two arms are withdrawn, so as to form but one line with the rest of the body.

The right arm put at rest, so that the hand is supported by the hip, indicates the termination of a word.

The left hand placed in the same position, is the sign of a comma, (,).

The two arms put in this position, signify a point and a comma, (;).

By putting the right arm into this position, and at the same time making the sign of *n* with the left hand, two points are indicated, (:).

By resting the left arm on the hip, and making the sign of *n* with the right arm, a point is indicated, (.)

By

By holding the right arm in such a position that the hand shall be above the head, or touch it, the point of interrogation is made, (?). Telegraphic signs by the hand figure.

The same motion performed with the left arm, furnishes a sign for the point of admiration, (!).

The three signs of the termination of a word, of a comma, and of the point of interrogation, each combined with the three different angles made with the other arm, form the nine figures.

By putting the right arm at rest on the hip, and forming the three angles of 45° , 90° , and 135° , with the extended left arm, the signs are given for the figures 1, 2, and 3.

By resting the left arm on the hip, and making the same three angles with the right arm, the figures 4, 5, and 6, are formed.

By bringing the right arm to the sign of the point of interrogation, and repeating the three angles with the left arm, the figures 7, 8, and 9, are formed.

To indicate zero, (0), the two arms are raised so that the hands shall be above the head, or touch it.

To express 10, the signs of 1 and 0 are made without making either the sign of the termination of a word, or of a number, or that of a comma, between these two signs. To express 11, the sign of 1 is twice made, with the assistance of the sign for the termination of a character, or of a figure. To indicate 12, the signs of 1 and 2 are made, and so on: 10, 11, and 12 may be expressed at pleasure, each by a single sign, by placing the left arm in the position of the point of admiration, and by successively forming with the right arm the three angles of 45° , 90° , and 135° . It would not be difficult to devise a great number of signs to express 13, 14, 15, and many other numbers, each by a single sign.

If it be intended to make use of this natural telegraph at the distance of one, or even of several leagues, or to do without a telescope at less considerable distances, it is only necessary to add to each natural arm an artificial one, that is to say, an oblong frame, of the breadth of a foot or thereabout, and of the length of one or more yards. This frame must be covered with oiled silk, or any other stuff of a dark colour, and must be provided with a handle to hold and direct it. Our telegraph would here no longer be called *natural*, but it is still among the Mechanical addition for great distances.

May be used by
night as well as
by day.

the most simple of the artificial. I am of opinion that this kind of telegraph may be employed with very great advantage: it is simple and cheap; it may be used in all directions, and is removed with ease. In this latter point of view, it would doubtless be well calculated for the formation of moveable telegraphic lines, or what might be called a flying telegraphy, which in my opinion would be very useful in war, to keep up a quick and constant communication between the different bodies, and with the fixed and common telegraphic lines. A machine of easy carriage, and calculated to raise a man to a considerable height, would, no doubt, contribute to render the flying telegraphy more perfect. The telegraphs with the frames may be used by night as well as by day, by adding lanterns to them, as in the common telegraphs. One must be placed in the middle, that is to say, on the breast of the person who forms the telegraphic signals with his arms and the frames, one at the handle, and one at the extremity of each frame. To prevent the person from being affected by the vapours of the lantern in the middle, it should be placed at a small distance before him; and when the movements are intended to be visible on both sides, it would be still better to place this lantern on the head, or to put one on each shoulder.

Perhaps this kind of telegraph might be advantageously employed to establish immoveable telegraphic lines, in every case in which the telegraphs employed at present would be too expensive. The same telegraph might frequently serve for several lines in different directions. By means of this telegraph a communication might be maintained, with little cost, even in small districts, or in the provinces and departments, with the capital or the chief place, and the other considerable points. Of what importance may it be to be able to indicate instantaneously conflagrations, inundations, or other events which require speedy succour.

It would be well to place the telegraph, or the person who makes the telegraphic signals, on a kind of stool, to facilitate his movements. Placed in this manner, with his arms and the frames he could make a prodigious number of figures, sufficiently distinct not to be confounded with each other.

VI.

*Account of a Fact respecting Water heated in a Boiler of Stone;
with Observations. By Mr. J. C. HORNBLLOWER.*

To Mr. NICHOLSON.

DEAR SIR,

THOUGH it must be admitted by every one who has attended to the critique contained in your note on the experiments of Count Rumford, in your first volume * of the Philosophical Journal, concerning the non-conducting power of fluids with regard to heat, that it is an erroneous conclusion, and which several experiments, particularly that of Mr. Murray, have since confirmed; yet I cannot be quiet without informing the gentlemen who have so constantly opposed the Count's doctrine, of a circumstance which accident brought forth on a very large scale, about forty years ago, in the county of Cornwall.

It had long been a desideratum in the mining interest in that county, to reduce the consumption of fuel in draining the mines by the means of steam engines, and every expedient that carried any tolerable face of probability was brought to a trial.

Among the rest it was suggested, that as, in the several operations of smelting the produce of the mines, much heat must be carried off, from the intense fires of their furnaces, which might be employed to some purposes requiring but a subordinate temperature; a resolution was formed by a company of gentlemen, with Mr. John Weston at the head of it (to whom mining was an ungovernable hobby-horse), to erect an engine on a copper mine in the parish of Camborne, and to put up a set of furnaces so attached to the engine that they might avail themselves of this superabundant heat to raise the steam.

To effect this they had their engine-boiler made of masonry, of what is called in Cornwall *moor-stone*, well wrought and put together with Aberthaw lime, which has the property of setting

Fluids not
strictly non-
conductors.

Incident.

Attempt to re-
duce the con-
sumption of fuel
in working
steam engines.

Trial to work
by the smelting
heat.

A stone boiler
connected with
water-lime, &c.
heated by cop-
per tubes.

WATER HEATED IN A BOILER OF STONE.

Dutch trafs *; and to convey the heat, two or three tubes were placed in it from end to end, and the furnace connected to one end of the boiler, and the engine at the other; which, though it was a prodigal way of saving fuel, yet it was competent enough to raise steam for the use of the engine in certain cases.

When the water boiled, a cock near the bottom gave water, but little heated,

At the bottom of the boiler was placed a cock, as usual, to open it as occasion required, either when it was necessary to clean it, or in case it should be over-charged with water, which I think was the case which brought the following fact to light; namely, that when the fire had been lighted, and the heat had to circulate through this mass of stone-work, and the water brought to boil, and the engine had been at work for the first time, which is usually attended with many unforeseen delays and hindrances, the cock being turned, the water was not hotter than to admit the hand without any painful sensation of heat. Many deep and grave hypotheses were formed on this extraordinary discovery, which, for the sake of the profession at this advanced state of science, I must forbear to mention. But at that period many a valuable truth had come and gone like a worthless mendicant, whom few regarded, and whom none would receive.

The tubes were near the surface, and the vessel a bad conductor.

The most striking circumstances in the detail of this discovery are, that the temperature downward (for the tubes were near the surface of the water) must be greatly accelerated by the conducting power of the sides of the boiler, and the agitation of the water in a state of ebullition: notwithstanding all, it must have occupied four or five hours to bring the lower part of the water to the temperature of 90° , or perhaps 100° ; and no doubt can be entertained but that they deemed it a matter of no consequence as to the place of the tubes, so long as they were covered with water; believing with every body else, that the whole mass would attain the boiling point at the same time.

Hail the man who throws if but one grain of truth into the scale against popular prejudice or vulgar error; and notwithstanding the Count did not obtain the extreme of precision in this particular instance, he has laid the foundation for all the subsequent experiments which have gone to confirm or reject

* This lime is found on the beach on some part of South Wales.

the position he advanced. This I may be permitted to observe, without the imputation of detraction from those whose labours and talents demand my respect.

I am, Sir,

Your very obedient servant,

City Road,

J. C. HORNBLOWER.

June 11, 1804.

VII.

Facts and Observations tending to elucidate the Theory of Galvanism. By A CORRESPONDENT.

THE pile of Volta has disclosed many new properties which were not known to belong to the two powers of the electric fluid, for by its operation, two sets of chemical phenomena are produced, differing essentially from each other, thereby determining the real or positive nature of each power. It has introduced electricity as a chemical agent, and has shewn us that it acts as such in its two-fold character very generally throughout nature. Electricity has hitherto been considered mostly as an adventitious power excited upon the surfaces of bodies, it is now known to attach itself to, and to enter into combination with various substances.

General effects
and theory of
galvanism.

If the spinal marrow of a frog be coated with tin foil, and the muscles of the lower extremities be laid bare, if the frog, thus prepared be drawn through water, at the bottom of which there is a piece of silver, when the tin foil touches the silver, the limbs of the frog are thrown into strong convulsions. If the tin foil be kept steadily at the first place of contact, the convulsions soon cease; but if it be removed to the smallest distance from where it first touched the silver, and is again in contact with it, the convulsions are renewed with their original violence. The same thing happens when the silver is connected with the muscles by means of a metal, though that metal touch the muscle in a point, as when the water acts as the conducting arch.

These experiments shew that there is a power in each assignable particle of the metal which excites the action of the muscles, and that nothing but the contact of the metals seems necessary for the production of the effect.

It

General effects
and theory of
galvanism.

If the power which excites the motion of the muscles be electricity, it is in a state not to be conducted from one particle to another, for the same effect is renewed whenever the tin-foil comes in contact with another part of the silver, however contiguous it be to the one at first touched. We find farther that the superficial particle does not take off the power of the one immediately beneath.

It is thus, I apprehend, that the renewal of surfaces gives a permanency to the powers of the pile, and in proportion to the quickness of succession in the presenting new surfaces, the power of the pile is found to increase.

It is needless to explain these appearances by the hypothetical principle of Mr. Lavoisier's oxygen, since Mr. Volta has shewn that the metals themselves are electro-motors, even without the intervention of water, or the action of acids or alkalis. Taking it for granted then that the animal muscles are a species of electrometer, we may conclude that some disturbance of electricity takes place when two metals are brought in contact, and a circuit is made. This disturbance varies according to the substances employed. Mr. Volta has found this to be the case, for he has rendered this small portion of electricity perceptible by accumulating it on his condenser. The pile of Volta accumulates the power which is called forth by the contact of two metals, and enables us to apply it in such accumulated state in various chemical experiments.

One of the most striking effects is the disengagement of two elastic aeriform fluids from water, by introducing wires, from which are produced the two states of the electric fluid. Thus it happens that the wire connected with the zinc produces, when immersed in water, one gas, and the wire connected with the silver or copper plate another gas. The properties of these gases are perfectly distinct, and evince that the producing powers have peculiar effects, for the water is common to both. What takes place from the wires in water at the extremities of the pile is more or less apparent between the plates themselves, where wetted paper is interposed, for there the action of the gases is to be seen in their effects on the metallic plates. The effect produced by the pile may be greatly increased by arranging it in the following order; silver, wet paper, zinc, a plate of glass, &c. and by placing wires on each side of the glass, and putting those into water; from each pair

pair of wires the two airs will be produced. Might not Mr. General effects and theory of galvanism. Wilkinson's experiment of burning charcoal be in this manner greatly increased in splendour?

In the prosecution of the present inquiry, I shall especially notice the two different effects produced in water by the wires of Volta's pile. It is known that these two wires throw out two powers similar to what are called positive and negative electricity; and as water is common to both, I am led to conclude that the two gases are produced in one instance by the union of one of these powers with water, and in the other by the other power and water, and to consider each gas as thus compounded, because we have no evidence of any other power, principle or substance being accessory to the production of either of the gases. Volta's pile causes combustion in atmospheric air, and therefore vital air is absorbed; it causes another species of combustion in vacuo, because the two principles of fire are supplied by it. Volta's pile does not act in vacuo, because the resistance necessary to accumulate the powers is withdrawn in the same manner as the Leyden jar does not charge in vacuo.

If the gas which is produced from one of the wires communicating with the pile in water be united and inflamed with the other in a just proportion, the water which is common to both is re-produced, and common fire is generated in great abundance; now as we have had no evidence of the presence of fire till this point of time, does it not appear that these two principles which are thrown out from the wires of the pile, are by their union transformed into ordinary fire, and does it not appear in this experiment that ordinary fire is generated by and compounded of these two powers?

Lavoisier has triumphed over the friends of Phlogiston. He overturned a system which was built upon the assumption of a principle, upon a mere name of hypothetical entity, endued with attributed or imaginary properties. But in the place of the system he destroyed, he has erected one of his own. Has he not built upon the same weak foundation with his late adversaries? Does not his system rest altogether upon the assumed distinct existence of things, the distinct existence of which cannot in any way be evinced? Do not all his solutions of chemical phenomena depend on such assumption? May not all these phenomena be explained with equal ease, nay more simply, by referring them to the action of known agents?

If

General effects
and theory of
galvanism.

If we examine Mr. Lavoisier's theory of combustion, we shall find that it depends upon the assumption of a principle whose existence has never been proved by direct experiment. It is the union of the basis of oxygen gas with the combustible body which in every instance is the cause of the combustion. We have seen that this gas is produced by positive electricity and water, and we conclude that the ponderable part of this gas is water, because we can perceive the agency of no other principle or substance. The simple condensation of oxygen gas, according to Mr. Lavoisier, occasions the evolution of heat and light. But little heat is excited by the mechanical condensation of this gas; but we find that when a chemical union takes place between a combustible and the gas, a great portion of heat and light is disengaged.

The accension of charcoal and metallic bodies by the galvanic battery, even in vacuo, seems to shew that the electricities thus produced are the peculiar agents in the phenomena of combustion. They seem not only to be the excitors of combustion, but they furnish with water those gases by which combustion has been conceived to be upheld.

The theory of the Lavoisierian school, of the combustion of hydrogen gas, in conjunction with oxygen gas, is as follows: "When both gases are mixed at a lower temperature than that of ignition or red heat, the attraction of their respective bases to the caloric is, in that case, stronger than their attraction to each other, and therefore they are not decomposed. But in the heat of ignition, their bases, namely *concrete* oxygen and hydrogen, again attract each other more strongly, and hence they unite to produce water; and both the caloric and light by which they are retained in their aerial form, are again disengaged from them, and constitute the fire."

Now where is the evidence of the heat and light being necessary to the formation of these gases, when they are produced from the wires connected with the extremities of the pile, and placed in water? Also during the oxidation of a metal in the moist way, the hydrogen discharged is at first very hot, so as to heat immoderately the tubes used in collecting it. If the condensation of oxygen gas be the cause of the production of heat and light, how comes it that charcoal and nitre desflagrate together with the production of so much light and heat, although the compound formed, namely, carbonic acid gas, occupies so much

much more space, than the substances from which it is formed? Does it not seem an assumption to say that the two conditions of oxygen in the aeriform state, and oxygen in the solid state, as in nitre, should hold the same quantity precisely of heat and light? This fact disproves the opinion that the production of heat and light is commensurate with the condensation that takes place in combustion. General effects
and theory of
galvanism.

An established law of chemical affinity would be reversed, if oxygen, when in combination with another substance, were to have a greater, or even the same attraction for the matter of heat as it has when oxygen is simply combined with the matter of heat.

There are many instances of combustion, deflagration, &c. wherein the compounds formed seem to hold more heat in their composition after than before combustion. When gun-powder is inflamed, does not the production of steam seem to shew that a great quantity of heat is *generated* during the explosion. If the matter of heat was held to the particles of the component parts of gun-power, in consequence of their great capacity for containing it, should we have any reason by analogy to suspect that the electric spark would entirely reverse the order of the existing attractions? Have we not evidence of the accumulation of one power of the pile in nitrous and muriatic salts, for in the formation of their acids we do not find combustion to be necessary, and there is no known expenditure of this their igneous principle. These are the salts which cause the combustion of inflammable bodies, and burn them, in consequence of their both containing igneous and inflammable principles. Thus does it appear that fire is generated during combustion.

From all the observations and experiments I have made on this subject, I conclude that water is still to be considered as a simple substance, that its two assumed component parts are non-entities, that fire is generated in every species of combustion, whether by acids or otherwise, and that heat is generated in the lungs during respiration. I take the liberty also to suggest, that the admission of the hypothetical substances of the Lavoisierian system may have retarded the progress of science, by diverting the mind from real objects. Analogies drawn from imaginary data must tend to perplex and confound, and thus do names arise without existing realities. Perhaps it would

would be as proper to question the existence of azote, if oxygen and hydrogen were to fail, as it was to admit it, from the supposition of the existence of those two substances.

A CORRESPONDENT.

VIII.

*Experiments with the Electric Pile, by Mr. Ritter, of Jena.
Communicated by Mr. ORSTED.**

WHILE the great inventor of the electric pile was proving the identity of galvanism and electricity, many of the philosophers of Germany were busied in the same pursuit. On this subject the celebrated Ritter undertook a very extensive series of experiments, the results of which are so remarkable as to merit attention, even after the publication of the labours of the philosopher of Pavia.

To compare galvanism with electricity we must attend to the kind, chemical action, spark and shock.

1. Kind.

The pile has a positive and a negative pole: and the intensities decrease gradually from the extremities to the centre.

But the whole pile becomes negative, when the positive pole is made to communicate with the earth by means of a conductor, and *vice versa*.

Yet this change in the kind of electricity does not affect its chemical action.

A collateral fact is the increase of the action by adding a salt to the water without changing the kind of electricity.

In order to compare galvanism properly with electricity, four different phenomena are to be distinguished; the kind of electricity, chemical action, spark, and shock.

As to the kind of electricity, it is known to every person, that the pile has two electrical poles, one positive, the other negative. On a more attentive examination we discover, what was not difficult to foresee, that the respective intensities are strongest at the extremities of the pile, and regularly decrease from the extremity to the centre, where the intensity is at its minimum. But it was never yet suspected, that the whole pile becomes negatively electrified, when a communication is made between the positive pole and the earth, by means of some conducting substance; and on the contrary, the whole pile becomes positive; when the electricity is abstracted from the negative pole. We have here a phenomenon, that shews the theory of electricity to be still in its infancy: When the state of the pile is thus changed, its chemical action is not destroyed, but continues as before. This fact is perfectly consistent with the augmentation of the chemical action of the pile, on the addition of salts, acids, or alkalis, to their wet strata, in which case the kind of electricity remains constantly the same as when simple water was employed.

* *Journal de Physique*, December, 1803, Vol. LVII, p. 401.

The assertion of Volta, since repeated by Van Marum and Pfaff, that the electrical pile charges a jar or a battery instantaneously, requires some limitation.

With regard to common piles, the assertion is true: but if the pieces of pasteboard, instead of being thoroughly wetted, contain but very little moisture, that of the damp air for example, the action takes place much more slowly. At first, indeed, the action is still tolerably prompt, but in proportion as the pasteboards lose their moisture, the action becomes gradually slower; so that a pile of six hundred pairs of zinc and copper, used immediately after it was made, was ten or fifteen minutes charging a battery of thirty-six square feet to the same degree, to which it would have charged it instantaneously, had it been constructed with pieces of pasteboard thoroughly wetted. Each of the wet strata too may be composed of a piece of glass, armed on each side with wet pasteboard; and such a pile of six hundred pairs would require twelve hours to charge the battery to the same degree, as would be done by a common pile, with a solution of a salt, in an imperceptible space of time. The law of this retardation therefore is, that the action of a pile is more slow, in proportion as it is a worse conductor.

Ritter has made a great number of experiments in particular, which prove, that the state of the pile, on all occasions, obeys the same laws, as that produced by the electrical machine: but we cannot here enter into particulars, without exceeding the bounds allowable to an abstract.

It is a well known fact, that electricity produces the same change in water as galvanism. Ritter has shown, that positive electricity, like positive galvanism, disengages from it oxygen gas; and that negative electricity, like negative galvanism, disengages from it hydrogen.

Inquiries into the action of the pile on metals have taught him, that its negative pole disposes them to combine with the hydrogen of water, as the positive pole disposes them to combine with oxygen. The hidrogenation has different degrees with respect to the same metal, as well as the oxidation. Silver with a large quantity of hydrogen assumes the state of gas; with a smaller quantity it remains solid. He also found, that the electricity does not produce oxygenations and hydrogenations in the humid way alone, but in the dry way also. The oxidation

Volta, Van Marum, and Pfaff, say, that the pile changes a jar instantaneously.

This is true with the common pile; but if the pasteboards be little moistened, the jar will be charged more slowly.

Such a pile of 600 pairs was 10 or 15 minutes charging a battery of 36 feet as high as it would have done instantaneously with thoroughly wet pasteboard. By interposing glass armed with wet pasteboard between each pair of metals, the time would be protracted to twelve hours.

The action of the pile therefore is quick in proportion to its conducting power.

Ritter has proved by many experiments that the electricity of the pile obeys the same laws with respect to kind as that produced by the machine.

2. Chemical action.

Galvanism and electricity produce the same change in water; each in the positive state extricates from it oxygen; in the negatives, hydrogen.

The negative pole of the pile disposes metals to

combine with the hydrogen of water; the positive with its oxygen.

The hidrogenation of metals has different degrees, as well as their oxigenation.

Silver highly hidrogenated becomes a gas: less so, remains solid. Galvanism hidrogenates and oxigenates metals in the dry way also.

If the positive pole of the pile be armed with gold leaf, and the negative with a bit of charcoal, on making the contact, the gold leaf will burn with a bright flame, and the charcoal remain unaltered: if the situation of the two be reversed, the charcoal will burn, and the gold melt.

The hidrogenation is less distinct: but if mercury, in a vessel of iron or platina, be placed in contact with the negative pole, and its surface touched by a conductor from the positive, a spot or circle of a powder different from the black oxide, which is produced under the opposite circumstances, will be found on the surface.

tion produced by the positive pole is easily observable. Nothing more is necessary, than to arm it with a leaf of gold, and the negative pile with a bit of charcoal; when, on bringing them into contact, the gold leaf will burn with a bright flame, and the charcoal remain untouched. If the charcoal be placed in contact with the positive pole, and the gold leaf in contact with the negative, the charcoal will burn, and the gold melt. The hidrogenation produced by the negative pole is less distinct, so that it is seldom perceived: but facts may be adduced however, that prove its existence. If a little vessel of iron or platina, filled with mercury, be placed in contact with the negative pole, on touching the surface of this fluid metal with the positive conductor, we obtain a point or circle of a powder very different from the black oxide of mercury, which is produced when the mercury is placed in contact with the positive pole, and touched with the negative conductor. The oxide produced in the latter case arranges itself in the figure of little stars, equal to those produced by positive electricity with the powder: and the circular figures on the mercury at the negative pole are likewise equal to those produced by the powder electrified with the negative conductor.

In a pile the poles of which are not made to communicate by means of a conducting substance, the chemical action of the strata composing it is very unequal. The plates of zinc are oxidized less in proportion to their distance from the positive pole; so that those nearest the negative pole have frequently no traces of oxidation, and seem rather to have been protected from the action of the water by which they are wetted, than attacked by the action of the pile. This may be rendered still more evident by placing every fifth pair in contact with an iron wire, the other end of which is plunged in water. In this experiment the oxidation of these wires will be in the inverse ratio of their distances from the positive pole; in the centre the wire will not be more oxidized than another simply plunged in water for an equal length of time, and all the wires beyond this will be still less oxidized. Hence it is evident, that another action, the reverse of oxidation, has taken place.

Of all the effects of the pile, its action on the human body has been least examined. The shock, or rather palpitation, that it excites, has been considered as too simple, to be subjected to strict inquiry; and the flash, as well as its action on the

the tongue, has drawn but slight attention. It is true these inquiries, like all others relating to organized beings, are very difficult, particularly when they concern an action that is frequently injurious to the living subject. Ritter has more than once paid for the following discoveries, by long and even dangerous fits of illness.

It is well known, that the skin, being a bad conductor, must be wetted, to make it a good one: it is likewise found in practice, that a surface of considerable extent must be wetted and armed with metal, to have all the possible effect of a pile. The reason is not difficult to discover, though it may lead to many important consequences: we have only to advert to the known fact, that conductors can only convey a quantity of electricity proportionate to their surface; whence it follows, that, to produce the greatest effect, a considerable extent of skin must be made a good conductor. If one of the surfaces wetted and armed with metal, which is touched by the conductors of the pile, be larger than the other, the sensation is less distinct than that which takes place on the smaller, where there is a more perceptible, and often painful sensation: so that we are masters of the magnitude of the effect that we would produce on any part of the body, a very important circumstance in employing galvanism medicinally.

The following is an application of what has just been said. All the difference between the shock obtained from the pile, and that received from a jar, arises from the different states in which we are when we touch them: if a very large pile be touched with dry hands, we experience the same sensation, as if we had touched a charged jar: on the contrary, if with hands thoroughly wetted and armed with metal we touch a jar previously discharged by dry hands, we receive a shock similar to that of the galvanic pile.

Ritter reduces all the effects of the pile on the animal body to expansions and contractions. By the positive conductor he has made several parts of the human body assume a greater bulk; and by the negative he has made the same parts contract. When the tongue is brought into contact with the positive conductor, applying the negative to some other part of the body, and they be all left a few minutes in this state, a little rising is produced on the tongue: the negative conductor, placed in contact with it in the same manner, produces a little

In the latter case the black oxide arranges itself in stars, as when electrified by the machine positively; and the circular figures of the former resemble those produced by electrifying the powder negatively. When the poles are not made to communicate by means of a conductor, the chemical action of the strata is very unequal. The plates of zinc are oxidized less in proportion to their distance from the positive pole; and those nearer the negative pole appear to have been protected from this action.

If every fifth pair be in contact with an iron wire communicating with water, the oxidation of the wires will be in the inverse ratio of their distance from the positive pole; the central wire will not be more oxidized, than one simply standing in water, and the wires toward the negative pole will be preserved freer from oxidation.

3. & 4. Spark and shock. The action of the pile on the human body has been least examined.

It is in fact most difficult, and Ritter has brought on himself long and dangerous fits of illness by his experiments. The skin, to form a good conductor, must be wetted; and this to a considerable extent, to have all the effect of a pile. If the skin be wetted and armed with metal in two places, and touched with conductors from the pile, the sensation on the smaller surface is most perceptible, and often painful: hence we can command the magnitude of the effect we would produce on the body. The difference between the shock from the pile, and that from the jar, arises from the state in which we are when we touch them. If we touch a large pile with dry hands, we receive a smart shock; but if with hands wetted and armed with metal, we touch a jar discharged previously by dry hands, we feel the same sensation as from the pile. Ritter reduces all the effect of the pile on the body to expansion and contraction; the positive conductor expanding, the negative contracting the part. The pulse is made stronger by the positive conductor; weaker by the negative. The expansion is followed by a sensation of heat, and *vice versa*. The action of the pile on the organs of sense depends on the nature of the organ; but the two poles produce the extremes of the sensation. The eye in the positive state sees objects red, large, and distinct; in the negative, blue, small, and more obscure. On the tongue the positive pole produces an acid taste; the negative, an alkaline. In the nose the negative produces an alkaline smell; the positive, that of oxygenated muriatic acid. In the ears the positive produces a grave sound; the negative, an acute.

The action of the pile on the organs of sense is modified by the particular nature of each; but it is remarkable, that the two poles of the pile produce in some sort the two extremes of each species of sensation. I have already observed, in the abstract I gave of Mr. Ritter's discoveries on light some time ago, that the pile produces in the eyes those red and blue colours, which are nearly the extremes of those of the prism; and if it were not so difficult to distinguish the violet from the blue, undoubtedly we should have nothing to wish in this respect. In these experiments, the eye in the positive state, while it sees every object of a red colour, sees them at the same time larger and more distinctly: in the negative state, on the contrary, it sees them at once blue, smaller, and less distinct than they usually appear.

Thus the expansive power of the positive pole, and the contracting power of the negative, seem to exert their action here likewise.

The tongue is equally affected by the pile: the acid taste produced by the positive conductor, and the alkaline by the negative are sufficiently known.

The effect of the negative conductor on the nose is an ammoniacal smell; that of the positive is a depression of the sensibility of the organ, similar to what is produced by the oxygenated muriatic acid.

The ears, touched by the positive conductor, hear a grave sound, and with the negative, a sound more acute.

These experiments require much care: to repeat them properly it is necessary to read the descriptions at large, which the author has given in different tracts, where the particulars are minutely detailed.

Ritter reduces all the effect of the pile on the body to expansion and contraction; the positive conductor expanding, the negative contracting the part. The pulse is made stronger by the positive conductor; weaker by the negative. The expansion is followed by a sensation of heat, and *vice versa*. The action of the pile on the organs of sense depends on the nature of the organ; but the two poles produce the extremes of the sensation. The eye in the positive state sees objects red, large, and distinct; in the negative, blue, small, and more obscure. On the tongue the positive pole produces an acid taste; the negative, an alkaline. In the nose the negative produces an alkaline smell; the positive, that of oxygenated muriatic acid. In the ears the positive produces a grave sound; the negative, an acute.

IX.

*Account of a Machine for laying Land level.**By Mr. DAVID CHARLES*.*

THIS simple machine, which is the invention of my Steward, Account of a machine for laying land level. and of which I have seen nothing similar, appears to me necessary, even in the most fertile parts of England, where the new system of drill-husbandry has been introduced, or even where there is any attention to the waste of time, or to the ease of cattle in the act of ploughing; in order to get rid of crooked or unequal ridges, without either a summer fallow by cross ploughing, or else by frequent repetitions of ploughing in the winter and spring, which the humidity of our climate will not allow in every kind of soil.

I reduced fourteen acres of land last spring to a perfect level, where the crowns of the ridges were above two feet higher than the furrows, and where they were crooked and of unequal breadths. Six acres of this is now under turnips, a crop that gives sufficient time to ameliorate the under-strata of soil that had perhaps never before been exposed to the influence of the sun and air; and by the adoption of the Northumberland mode of sowing that root on dunged drills, it is almost immaterial where the upper strata is, provided the seed vegetates, as it soon strikes into the manure, and rapidly flourishes.

My chief success, however, has been upon a field of eight acres, which lay in the unprofitable state already described. This land, which is a deep clay, and which had produced a crop of wheat from an old lay sod the former year without any manure, was winter ploughed, and lay in that state until the leveller was introduced the first dry weather in April. It was preceded by two horse-ploughs, taking perhaps a square of an acre at once: these loosened the soil the depth of a common furrow, and twice the breadth across the ridges. The leveller followed, drawn by two oxen and two horses, with a man at each handle, to press it down where the weight is to be removed, and to lift up the body by the handles

* From the Transactions of the Society of Arts, who rewarded the Inventor with their silver medal. The communication was made by Lieut. Col. Hardy, of Westmead, Carmarthenshire.

where

Account of a
machine for lay-
ing land level.

where it is to be discharged. Thus four men, one driver, and eight head of cattle, will more effectually level from half an acre to three roods in one day, according as the earth is light or heavy, than sixty or eighty men would accomplish with barrows and shovels, &c. even with the assistance of a plough. In sandy ground where the depth of one furrow will bring all to a level, as much will of course be done in one day as two ploughs can cover; but my ground required to be gone over several times. After this field was levelled, the backs of the ridges, as they are termed, which were stripped of their vegetable mold, were ploughed up, the furrows not requiring it. They were also harrowed, and the field copiously manured with lime compost; harrowed in, and broke into nine-foot ridges, perfectly strait, in order to introduce Duckitt's drill. It was sown under furrow, broadcast, the last of it not until the 13th of May, and was cut down a reasonable crop the 4th of September. I am now thrashing it, and a sample shall be sent, as well as a return of the eight acres if necessary.

The field now lies in proper form, well manured, with the advantage of a fair crop from heavy tenacious ground, without losing a season, and in a year by no means favourable.

I am well aware there are many shallow soils, where it may be hazardous to remove the enriched surface, and trust perhaps one half of your land for a crop that had never before been exposed to the atmosphere; but where the soil is sufficiently deep, or you have good under-strata, and there is manure at hand to correct what is sour from want of exposure and tillage, it is evident from this experiment that no risk is run.

To avoid the expence of a fallow, and to lay out ground in strait and even ridges, even where drill husbandry is not practised, should be objects to every rational farmer. But where the new system is intended to be adopted, it becomes indispensably necessary. In laying down lawns, parks, &c. where furrows are an eyesore, or places inaccessible to wheel carriages from their declivity, and from which earth is to be removed, it will be found equally useful.

Should the society consider the inventor, David Charles, worthy of any remuneration, honorary or otherwise, it will be gratefully acknowledged by

Your obedient Servant,

Westmead, Jan. 1, 1803.

JOSEPH HARDY.

Certificates

Certificates from Mr. Owen Edwards, of Brook, and Thomas Bynan, carpenter, of Westmead, accompanied the above letter, confirming the statement made herein.

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ing land level.

Description of the Machine for laying Uneven Land level, invented by Mr. DAVID CHARLES.—Plate XI. Fig. 1, 2.

Fig. 1.—A, Part of the pole to which the oxen or horses which draw the machine are fastened, and which is attached to the machine by a pin at B.

CC, The two wheels, shod with iron, which run upon the axle D.

EE, The upper frame-work of the machine, extending from the axle to the extremity of the handles FF, and secured firmly by the cross pieces.

GG, The curved iron sliders of the machine, which may be raised or depressed a little by means of the pins HH, which pass through holes in the wood-work, and also in the iron sliders; these sliders form one piece with the back iron scraper I, in the manner more fully explained in *Fig. 2*.

K, The wooden back of the machine, which should be made strong, to resist the weight of the earth when collected therein. The iron scraper should be firmly secured to this by screws and iron-work.

LL, The wooden sides of the machine firmly connected with the back and frame-work, in order to assist in collecting the earth to be removed.

M, A strong cross piece into which the ribs which support the back are well morticed.

Fig. 2.—K, The interior part of the back of the machine.

I, The iron scraper, sharp at the bottom, and firmly screwed to the back of the machine.

GG, Parts of the side irons or sliders, showing the mode in which they are united with the scraper I.

M, The cross piece above described.

X.

Experiments on Magnetism; by Mr. RITTER, of Jena. Communicated by Dr. ØRSTED, of Copenhagen.*

The phenomena of magnetism and electricity have often been compared, but we want facts.

THE phenomena of magnetism have frequently been compared with those of electricity, and many facts seem to justify the comparison. These facts, however, are neither numerous enough, nor sufficiently conclusive, to compose a complete theory. A series of experiments, exhibiting the magnetic needle in all its relations to electricity, at present better known by means of the pile, would undoubtedly throw much light on

Ritter has instituted new inquiries on the subject.

His experiments did not always answer, but exhibited many interesting facts.

a subject heretofore so obscure. Ritter felt the importance of such an undertaking, and began fresh inquiries concerning magnetism, with the same ardour and sagacity that have ever distinguished his labours. Though these experiments did not always answer the full extent of his designs, they notwithstanding exhibit a sufficient number of interesting facts to excite the curiosity of every natural philosopher.

A magnetic wire, and another not magnetic, excited galvanic palpitations in frogs; the south pole more strongly, the north less, than iron not magnetic.

Hence he inferred, that the south pole has a greater affinity for oxygen; the north, less.

This confirmed by experiment. Magnetic iron wires being placed in weak nitric acid, the south pole was most oxidized. The south pole of a magnetic wire being immersed in one phial of water,

Mr. Ritter's first experiments with the magnet were on frogs. He found that a magnetic iron wire, with another not magnetic, excited a galvanic palpitation in these animals. Presently he observed, that the south pole excited stronger palpitations, and the north pole weaker, than the iron not magnetic. Having constantly noticed, that the metals most susceptible of oxidation excited the strongest palpitations, he inferred, that the south pole possesses a greater affinity for oxygen than simple iron, and the north pole less.

This supposition he confirmed by means of several chemical re-agents. He placed a magnetic iron wire on pieces of glass in a plate of earthen ware, and poured upon it very weak nitric acid. The south pole was attacked by the acid much more powerfully than the north; and was soon surrounded by a deposition of oxygen, the quantity of which greatly exceeded that of the other pole.

The different oxidability of the magnetic poles is very well exhibited likewise, by taking three small bottles of equal size, filled with water, either pure or slightly acidulated, and putting

* Journal de Physique, December, 1803, Vol. LVII. p. 406.

into one the south polar end of a magnetic wire, into a second the north polar end of a similar wire, and into the third the end of an equal wire not magnetic: the south pole will first begin to deposit oxide, the unmagnetic iron a little after, and the north pole last. This experiment requires considerable care. The surface of the water must be covered with very fresh oil of almonds, to exclude all access of air. Care must be taken too, that one of the bottles is not more exposed to the sun than the others, because light accelerates oxidation. Ritter convinced himself of this by direct experiments; exposing two iron wires in water to the sun, but covering one of the phials with black paper, when that in the phial left uncovered was oxidized much the more quickly.

the north pole of one in another, and the end of a simple wire in a third; the south pole first deposits oxide, and the north pole last. This experiment requires particular precautions. Air must be excluded; and light accelerates oxidation.

If infusion of litmus be substituted instead of the water in the three phials in the preceding experiment, the relative oxidations will be the same; but they will be attended with a change of colour, shewing, that an acid is produced proportional to each oxidation; so that the south pole not only undergoes the greatest oxidation, but likewise reddens the infusion of litmus most *. The action that takes place in this experiment is very feeble, and frequently requires a week or more to produce a distinct effect; and indeed to accelerate it so much as this, it is necessary to add, previously to the infusion, as much acetic acid as will incline it to red, without completely changing its colour. The infusion reddened in this experiment resumes its blue colour on exposure to the air: but we must not hence conclude, that the acid produced by the action of the magnet is very volatile, for infusion of litmus reddened by phosphoric acid, or any other, exhibits the same phenomenon.

If infusion of litmus be used, the relative oxidations will be the same; but an acid in proportion to each will be produced, as appears by the change of colour. This action is very slow; and to accelerate it a small portion of acetic acid should be added to the infusion. The infusion thus reddened becomes blue again on exposure to the air: but it is the same when reddened with any other acid. Experiment.

The following experiment exhibits some things peculiar, and therefore I shall give it more at large. It has not been repeated, but the harmony of its results is in favour of its accuracy. Sixteen magnetic wires, of equal size and power, were placed in six vessels, all equally full of a mixture of one part nitric acid and thirty-six parts water, in the following

* Ritter has remarked, that the oxidation of zinc, and several other metals, in pure water, produces an acid. Mr. Jager, a celebrated physician of Stuttgart, made the same discovery, without knowing any thing of Ritter's.

Ritter and Jager have found, that the oxidation of several metals in pure water produces an acid.

manner:

In phials containing water 36 parts, nitric acid 1 part, were immersed: 1. a N. and S. pole, at $\frac{1}{2}$ a line distance: 2. the same $1\frac{1}{4}$ inch: 3. 3 S. poles, at $\frac{1}{2}$ a line: 4. the same at $1\frac{1}{2}$ inch: 5. 3 N. poles, at $\frac{1}{2}$ a line: 6. the same at $1\frac{1}{4}$ inch. The oxide deposited in these with respect to quantity was in the following order: No. 2, 1, 3, 4, 5, 6. The loss of fluid by evaporation, was in the inverse ratio of the oxidations. All the magnets were weakened in power: No. 2 least; 1 more: in all the rest one wire was weakened more than the others. On immersing two south poles in one phial, two north in another: the latter were most oxidized. Mr. R. attempted unsuccessfully to construct a battery with 120 magnetic wires, their poles placed in opposition, and separated by a drop of water; but he has not given up the design: and means to pursue his inquiries on the subject at large very extensively.

manner: In the first glass were placed two wires, one with the north pole immersed in the fluid, the other with the south, and not more than half a line asunder: In the second, the same, but the wires an inch and three quarters apart: In the third and fourth were each three wires, with the south poles of all immersed, but their distances in the two glasses different, as in the first and second: In the fifth and sixth were wires similarly arranged, but with the north poles immersed. Different quantities of oxide were gradually deposited; and to express the whole in few words, we will call the south pole S, the north pole N, their greater distance *g*, and their less *p*; and we will express the order of oxidations as follows: $S N g > S N p > 3 S p > 3 S g > 3 N p > 3 N g$. On the nineteenth day it was observed, that the loss of fluid by evaporation had not been equal in all the vessels, but took place in the inverse order of the oxidations. All the magnetic wires were weakened in power; $N S g$ least; $N S p$ more: of the wires $3 S p$, two had lost less power than the third; and in like manner $3 S g$, $3 N p$, $3 N g$, had each two left more powerful than the third; the strongest were equal to $N S g$.

In another experiment, where two little vessels filled with infusion of litmus were employed, one of them containing two magnetic wires, the south poles of which were immersed in the fluid; the other two similar wires, of which the opposite poles were immersed; the oxidation was greatest in the latter vessel*.

Lastly, Mr. Ritter endeavoured to construct a battery of magnets, but he did not succeed. For this purpose he employed a hundred and twenty magnetic wires, placed so that each pole had its contrary opposite to it, and separated from it by a drop of water; but this apparatus produced no effect. The ingenious author, however, has not relinquished the hope of being able to compose a magnetic battery, though other experiments, not less important, have hitherto prevented him. This series of experiments he considers only as the commencement of a very extensive labour, the results of which we hope soon to obtain.

* This appears contradictory to the experiment adduced in the third paragraph.

T.

XI.

*Observations on Arseniated Copper. By HAUV *.*

THE only ores of arseniated copper which are well known, History of the discovery of the ores, and their composition. are those from the county of Cornwall in England. The determination of their true composition followed closely on the discovery of this metallic substance, for which we are indebted to the fortunate chance which threw some specimens into the hands of the celebrated Klaproth. It was in 1787 that he published in the *Journal de la Société des Curieux de la Nature* †, the result of the examination which he had made of this new mineral.

The authors who have spoken of arseniated copper since that period, had only described it under the form of acicular crystals, when Citizen Lelievre, member of the Council of Mines, having suspected the existence of a peculiar substance, from the inspection of a group of green hexagonal bevelled laminæ which were given to him, made an essay of it, and discovered the presence of oxide of copper and arsenic acid. Citizen Vauquelin soon afterwards confirmed this indication, and determined the proportion of the relative quantities of the two principles contained in the same substance.

About this time the opening of a second mine in the county of Cornwall occasioned the re-appearance of arseniated copper, the vein which had been formerly explored being exhausted. This discovery was the more important, as the substance appeared, in its new situation, with characters altogether peculiar, and under forms hitherto unknown.

M. de Bournon, who was at hand to participate in this increase of riches which resulted to mineralogy, sent to Citizen Gillet Laumont and me, several specimens chosen from among those he possessed; and that which added to the value of his gift, was his haste to communicate to us the interesting work which he had prepared, on the crystallography of arseniated copper, before he published it.

* Translated from a pamphlet in quarto, sent by the author to the Count de Bournon; probably forming part of a journal.

† Tom. VIII. p. 160.

Mr. Chenevix was employed at the same time on the analysis of this substance. Soon after they both published the results of their researches in the *Philosophical Transactions**; and Mr. Chenevix testifies his admiration at such a perfect agreement between two sciences which employ two methods so different to interrogate nature. M. de Bournon, on his part, says that the analyses of Mr. Chenevix have given the most satisfactory sanction to the division which he had himself made, of the arseniated copper into four distinct species.

Description of
the varieties.

Before going farther, it is necessary to make known the varieties of arseniated copper which I have been enabled to examine. I shall confine myself to giving a description of them, succinct and independent of the laws to which the structure of crystals is submitted, the actual state of our knowledge on this subject only admitting of hypothetical views, of which I shall speak hereafter.

1. Obtuse octahedral arseniated copper, (*Plate IX. Fig. 2*): incidence of P on p , $50^{\circ} 4'$; of P' on p' , $65^{\circ} 8'$; of P on P' , $139^{\circ} 47'$ †. The colour of the crystals is sometimes a fine celestial blue, and sometimes a green, which varies between a grafs-green and a pale green. The octahedron sometimes becomes cuneiform, lengthening so that the terminal edge is parallel at D .

2. Lamelliform arseniated copper. In hexagonal laminæ, whose narrow faces are inclined alternately in contrary directions; the incidence of two of the narrow faces, situated on the same side, on the correspondent base, 135° nearly, according to M. de Bournon: incidence of the third on the same base, 115° nearly.

* For 1801, p. 199, *et seq.*

† I adopt here, very nearly, the results of M. de Bournon, who indicates 50° for the incidence of P on p , and 65° for that of P' on p' . I have only endeavoured to find limits capable of facilitating the calculations which I propose to make. Let bac , gac , (*Fig. 4*) be the same faces as P and P' , (*Fig. 2*); let ao (*Fig. 4*) be the height of the pyramid which has its summit in A , (*Fig. 2*); on (*Fig. 4*) a perpendicular to bc , and or perpendicular to cg :—if $ao = \sqrt{588}$, $on = \sqrt{2695}$, and $or = \sqrt{1440}$, we shall have $50^{\circ} 4'$ for the incidence of P on p (*Fig. 2*), and $65^{\circ} 8'$ for that of P' on p' ; whence is deduced, by calculation, $139^{\circ} 47'$ for that of P on P' .

The

The laminæ divide parallel to the large faces with great ease. Their colour is a fine grass-green.

3. Acute octahedral arseniated copper, (*Fig. 3*): Incidence of r on r' , 96° , according to M. de Bournon; of l on l' , 112° . The colour is a brown green more or less deep.

a, cuneiform. The preceding octahædron lengthened so that the terminal edge is parallel to π . This form, which is the most common, offers the appearance of a long rhomboidal prism, more or less acute, and is terminated by dihedral summits.

4. Trihedral arseniated copper. In a strait triangular prism, which is at the same time equilateral, according to M. de Bournon.

When the crystals have not been long exposed, their colour is a fine, bluish green; but their surface is subject to change and take a blackish tinge. If they are scratched, their primitive colour will re-appear.

5. Capillary arseniated copper. This is properly the olivenerz of the German mineralogists.

6. Mammellated arseniated copper. In mammellated masses, striated in the interior. These two last varieties are susceptible of a great diversity of tints, which shew the transitions from grass-green to olive-green, to greenish-brown, to tawny (*mor-doré*), to yellow, to bluish, and to white, which is frequently fatty.

The following is the manner in which M. de Bournon classed the different modifications which have been just mentioned, according to the differences which they offer with respect to form, specific gravity, and hardness. M. de Bournon's classification.

He divides them, as I have said, into four distinct species. Four distinct species.
The first is derived from the obtuse octahedron: the type of the second is the lamelliform crystal, in hexagonal laminæ, with bevels inclined alternately in contrary directions. He takes the acute octahedron for the primitive form of the third, and connects the acicular crystals and mammelary concretions with it, as varieties: in the fourth he places the equilateral triangular prism, and several other forms which offer the same prism, truncated on its solid angles or on its edges.

On the other hand, Mr. Chenevix has given six results of the analysis of arseniated copper, which I shall detail, disposing them conformably to the order established by M. de Bournon. Mr. Chenevix's analyses.

First

ON ARSENIATED COPPER.

First Species, in oblique octahedra.

Oxide of copper	-	-	-	49
Arsenic acid	-	-	-	14
Water	-	-	-	35
Loss	-	-	-	2
				100

Second Species, in lamelliform Crystals.

Oxide of copper	-	-	-	58
Arsenic acid	-	-	-	21
Water	-	-	-	21
				100

Third Species, in acute octahedra.

Oxide of copper	-	-	-	60
Arsenic acid	-	-	-	39.7
Loss	-	-	-	0.5
				100.0

Variety of the same Species, in capillary Crystals.

Oxide of copper	-	-	-	51
Arsenic acid	-	-	-	29
Water	-	-	-	18
Loss	-	-	-	2
				100

Another Variety, in nubbellated Concretions.

Oxide of copper	-	-	-	50
Arsenic acid	-	-	-	29
Water	-	-	-	21
				100

Fourth Species, in trihedral Prisms.

Oxide of copper	-	-	-	54
Arsenic acid	-	-	-	30
Water	-	-	-	16
				100

Toward

Towards the conclusion of his memoir, Mr. Chenevix remarks that the natural arseniate of copper exists in three different combinations, the first of which contains 14 per cent. of arsenic acid, (first result above); the second contains 21, (second result); and the third 29, (third, fourth, fifth, and sixth results). It is true, the third result gave 39.7 of acid in the 100 parts, but as the remainder of the mass was composed of 60 parts of copper without water, he found that the proportion of the acid with the copper did not differ much from that which takes place in the varieties in which water forms a part: this induced Mr. Chenevix to comprehend this result in the same division. Yet he considers this combination as the only true arseniate of copper, while the other three are arseniates of hydrate of copper.

I feel the value of the double work from which I have given this extract so much the more, because, having read the memoirs which contain the developement, I am enabled to judge of the advancement which it has produced in our knowledge on a subject which was in a great measure new when MM. de Bournon and Chenevix began to be employed with it. The exposition which I shall add of some enquiries I have made on the crystallization of arseniated copper, and of the reflections which they have given rise to, have no other object but that nothing may be neglected which tends to elucidate in a greater degree, every thing connected with an object of such importance as the distinction of mineralogical species.

After having read the crystallographic part of the work in question, I was desirous to know if it was not possible to bring some of the crystals, described by M. de Bournon as belonging to different species, to the same form of the integrant molecule; but not being able to make all the direct observations which would have guided me in this enquiry, I was obliged to confine myself to simple hypotheses.

I therefore considered the obtuse octahedron as performing the functions of the primitive form; and I was the more warranted in conceiving this opinion, because the celebrated Karsten, in a supplement to the excellent memoir which he had published before*, on the combinations of copper with different principles, says that the octahedron in question is lamellated,

* *Journ. de Physique*, Brumaire, An. X. p. 342, et suiv.

Comparison of
the acute and
obtuse octahe-
dra.

in a direction parallel to the faces of the two pyramids, of which it is the aggregate*. Proceeding on this datum, I was curious to know if it was not possible to connect with the form of the obtuse octahedron in question, that of the acute octahedron, which M. de Bournon has taken for the type of his third species. Let P, P' (Fig. 2) be still an obtuse octahedron, in which the incidence of P on p is reckoned to be $50^\circ 4'$, and that of P' on p' , $65^\circ 8'$, conformably to the measures indicated above: if we imagine another octahedron (Fig. 3) the sign of

which is $\frac{DF}{lr}$, we shall find that the incidence of l on r is

109° , and that of r on r' is $93^\circ 36'$. Now the correspondent incidences determined by M. de Bournon, are one 112° , and the other 96° ; which in one case makes a difference of 5° , and in the other of $2^\circ 24'$.

The differences
in the angles ac-
counted for.

If the measures had been taken on crystals so well defined, that the differences could be considered as real, we must have concluded that they formed two distinct species, because even these differences could only have been done away by supposing the laws of decrement to be much too complicated to be admissible.

But if the crystals were not capable of very accurate measurement, we can the better conceive that the differences were simply apparent, and that it may be possible that the error did not wholly arise in one observation, since it was necessary to make two, in which small deviations might have been produced on opposite sides; and then mechanical division alone, by giving different results with respect to the two octahedra, would have shown that a conformity between the angles observed and those measured, would be purely accidental.

Comparison of
the lamelliform
variety with the
obtuse octahe-
dron.

I afterwards compared the lamelliform variety with alternate bevels, which is the second species of M. de Bournon, with the same octahedron with obtuse summits. Now, if we suppose two intersecting planes, parallel to the face P' , and which meet the center, they will detach an octahedral segment which cannot be supposed to have much thickness, and whose two large faces will be hexagons, and the six lateral faces, trapeziums,

* *Journ. de Physique*, Pluviose, An. X. p. 131.

inclined

inclined to the great faces *. But these trapeziums will not be situated alternately in opposite directions. The three which will form obtuse angles with the large faces, will be contiguous to each other, and the same will be the case with those which form acute angles with the same faces. For example, those of the trapeziums which form obtuse angles with the large face analogous to P' , will correspond to the two faces of the adjacent octahedra at B, B' , and to the face situated behind A , parallel to P . The inclination of this latter face on P is, according to M. de Bournon, 115° ; and the two others, as I have indicated them above according to my calculations, are each about $139\frac{1}{2}^\circ$.

Now, of the three lateral trapeziums in the lamelliform arsenical copper, one has the same inclination of 115° on this base, according to M. de Bournon, and the two others have 135° ; an estimate which he only gives as an approximation, and which only differs by $4\frac{1}{2}^\circ$ from that corresponding to it in the obtuse octahedron, (*Fig. 2*).

The great difference consists in this, that the three lateral trapeziums which look towards the same base, in the octahedral segment I have described, are contiguous to each other, as I have said; while those of the lamelliform arseniated copper alternate with the three others which look towards the opposite base †.

But there is a method of removing this difficulty. Let us conceive that the two sections made in the octahedron (*Fig. 2*), instead of being parallel to the face P' , are so to the face P . In this case the lateral trapeziums, situated on the two sides of the edges B, B' , will always have an inclination of $139\frac{1}{2}^\circ$ to the superior base. Now, if the segment parallel to p makes an angle of 115° with the base analogous to P , the three segments will preserve, with respect to those turned towards the opposite base, the alternation indicated by M. de Bournon.

* Several substances, among others the spinelle, offer examples of similar segments.

† The figure given by M. de Bournon, of which that is a copy in *Plate XI. Fig. 5*, seems to have been traced according to the condition that the three trapeziums turned towards the same base should be contiguous. This was doubtless an oversight of the draughtsman.

But the incidence of p on P gives, on the contrary, an acute angle of 50° . Now, let us imagine a decrement indicated by D , which acts on the face p and on that which is opposite; the faces produced will be situated vertically; whence it follows that that which will mask the face p will form an angle with P equal to 90° plus 25° , which is the half of the inclination of p to P , that is to say, the angle in question will be 115° , conformably to observation*.

Their division into four species is not admitted by the laws of structure.

I shall not urge these results farther, which, as I have already stated, I only offer as purely hypothetical; and I shall abstain from adding my opinion with respect to the fourth of the species admitted by M. de Bournon, which, according to him, has the equilateral triangular prism for a primitive form. It is enough for me to have shown that, with respect to the division of arseniated copper into four distinct species, the laws of the structure may give rise to doubts which deserve some attention. If they can be removed, as it is not impossible they may, another proof will be obtained in favour of an opinion on which no obscurity should remain, that it may be worthy of being unanimously adopted.

Comparison of Mr. Chenevix's analyses and M. de Bournon's division.

If we now consider the results of the analyses which Mr. Chenevix has made of the different modifications of arseniated copper, we find, that in such of these analyses as have had for their object the types of the four species admitted by M. de Bournon, three have given sensible differences in the relative quantities of copper, arsenic acid, and water. These analyses correspond with the first, third, and fourth species. Another analysis, made on the second species, gave only copper and arsenic acid, without water. Thus, supposing that the relations between the quantities of the three principles contained in the modifications which Mr. Chenevix calls *arseniated of hydrate of copper*, constitute true limits, and that, in the modification which he calls simply *arseniate of copper*, the absence of water depends on the nature of the substance itself; we shall, in this respect, find an agreement between the results of

* I have a lamelliform crystal, on which, instead of a simple bevil, there are two, situated in contrary directions on the sides of the same edge; but they are too small for it to be possible to determine the positions exactly.

analysis and those of crystallography, very favourable to the subdivision of the mineral in question, into four distinct species.

But Cit. Vauquelin, on analysing a piece of lamelliform arseniated copper, whose crystals were quite fresh, obtained a very different relation between the quantities of the three principles*. His result was as follows :

Oxide of copper	-	-	-	39
Arsenic acid	-	-	-	43
Water	-	-	-	17
Loss	-	-	-	1
				100

It is remarkable in this case, that the quantity of acid exceeds that of the copper, while in the result obtained by Mr. Chenevix it forms only a little more than a third of the quantity of copper. It is not therefore evident that the limits indicated by this celebrated chemist are essential to the substances analysed.

The experiments of the same philosophers on the capillary and of the acryls, offer differences not less striking. According to Cit. Vauquelin these crystals contain,

* Mr. Chenevix, in his memoir, gives a passage from a letter which Citizen Vauquelin had written to him, and in which he informed him, that having analyzed crystals of the lamelliform variety, he found that they were composed of 59 of oxide of copper and 41 of arsenic acid. Mr. Chenevix adds, that the great difference between this result and that which he had himself obtained from the same substance, induced him to repeat his analysis with great care and attention, and that he constantly found the same proportions of oxide of copper, arsenic, and water. It is very probable that this result announced by Cit. Vauquelin, and so different on the other hand from that now given, was obtained in a first essay, or that this chemist, when he wrote to Mr. Chenevix, trusted to his memory, which was not so faithful as it generally is. However that may have been, the only result avowed by Cit. Vauquelin is that we now publish, and which he has inserted in the *Journal des Mines*, No. 55, p. 562.

ON ARSENIATED COPPER.

Silex	-	-	-	-	2
Water	-	-	-	-	5
Arseniate of iron	-	-	-	-	7 to 8
Arseniate of copper	-	-	-	-	86
					100

This chemist adds, that if the arseniate of copper did not contain any foreign matter, it would be formed of about 69 parts of oxide of copper and 31 of arsenic acid.

Klaproth's analysis.

We have another result on the same subject, obtained by M. Klaproth, whose labours have concurred so advantageously with those of Vauquelin, to procure an exact knowledge of the composition of minerals. His result gave,

Oxide of copper	-	-	-	50.62
Arsenic acid	-	-	-	45.00
Water	-	-	-	3.50
Loss	-	-	-	88
				100.00 *

The quantity of copper is nearly the same as in Mr. Chenevix's result; but on one side we find 45 of acid with 3.5 of water, and on the other only 29 of acid and 18 of water; which is very different.

Mr. Chenevix's results do not agree with of those of M. de Bournon.

Besides, we need only keep to Mr. Chenevix's own results to find difficulties and causes of uncertainty; for while this celebrated chemist obtained a very sensible quantity of water from the capillary crystals and the mammellated masses, these two modifications were considered by M. de Bournon as simple varieties of the third species, which is the acute octahedron, and which gave only copper and arsenic acid without water. Further, if the analyses of the capillary crystals and of the mammellated masses, are compared with that of the crystals in trihedral prisms, it will be seen that the differences do not exceed those which are frequently met with between the analyses of several pieces which evidently belong to the same species of mineral.

I add, that M. de Bournon seems to have had more authority for considering the capillary crystals and the mammel-

* *Additions à la Connaissance chimique des Minéraux*, p. 192.

lated concretions as simple varieties of the acute octahedron, since he indicates the intermediate modifications which connect these varieties with their type; so that, according to him, there are crystals which are perfectly determined in one part of their length, and fibrous at their extremity.

M. de Bournon, doubtless struck with the exception which the agreement between the two sciences, announced by Mr. Chenevix and himself, seemed liable to, in this instance, has since inserted in Mr. Nicholson's Journal * a note, in which he proposes to establish a fifth species of arseniated copper, composed of the capillary crystals and mammellated masses, which seems to operate less in removing the difficulty than in bringing it to light.

A fifth species proposed.

It cannot be denied that the modifications of arseniated copper offer sensible differences in their aspect, their exterior forms, and their colours. M. de Bournon also indicates some in their hardness and in their specific gravities. But the reduction of natural beings to the smallest possible number of species, really distinct, is an advantage of such value to science, which it perfectionates by simplifying it, that, before separating substances, according to those diversities which seem to be at variance with the relations which they otherwise have, and before seeking particular specific names for them, which would be necessary, all the means of ascertaining that the diversities in question are not purely accidental, should be exhausted. Even though the researches which still remain to be made for the accomplishment of this object, should have no other effect but to cause the disappearance of one single species, admitted by the two celebrated men whose results I have set forth, from the system, they will not be unprofitable to the progress of mineralogy.

External differences should not be admitted alone as evidence of a different species.

* * A reply communicated by the Count de Bournon will appear in our next.

• Philosophical Journal, new Series. Vol. VII. p. 577.

Observations

XII.

Observations upon the Doctrine of Count Rumford respecting the want of direct conducting Power in Fluids with regard to Heat. By CIT. BERTHOLLET.

(Concluded from page 140.)

Nicholson, Thompson and Murray have proved, that heat passes through the particles of fluids; that currents are often imaginary; and that fluids differ in conducting power.

I AM of opinion, that the experiments of Nicholson, of Thompson, and of Murray, leave no doubt on the communication of heat between the particles of liquids: some of them show that the motions of the solid corpuscles which are agitated in a liquid, may often mislead, with respect to the currents which are believed to be perceptible: but their existence must not, for this reason, be denied, when a difference between the specific gravities is suddenly established, and when the heat is communicated at the lower part of a vessel. The others prove that the communication of heat may be made through a liquid in which no current can be supposed to transport it immediately to a solid body, and they prove that liquids are possessed of a conducting faculty which differs in its intensity; but it is not to be inferred from this, that the locomotion of the particles of liquids does not contribute to establish an equilibrium of temperature more speedily: it is even probable, that the latter effect is generally the greatest.

These general facts account for all the phenomena.

The foregoing considerations, into which I have admitted the application of the faculty of communicating heat common to all bodies, of the conducting difference, and of the more speedy distribution of heat by means of the difference of the specific gravity which it introduces between the particles of a fluid, seem to me to account for all the phenomena which the discernment of Rumford has made public.

That gases receive heat very rapidly, is seen in the expansion of air thermometers and air balloons.

These considerations lead me to an opinion very different from his; it is known with what rapidity the thermoscopes, or air thermometers, indicate the variations of temperature: Pictet could not observe a second of difference between the elevation of a thermometer of this description, and the emanation of radiant heat by a body placed at a distance: it has been observed, that aerostats experience a sudden dilata-
tion

tion by the appearance of the sun*; these phenomena seem to me to indicate that the elastic fluids, far from being bad conductors, on the contrary, receive the temperature of other bodies very quickly; for, can it be supposed that all the particles of the gas take the temperature which they acquire by the contact of the covering of the balloon alone, and how can it be conceived, that the lower particles, which are contiguous to that portion of the covering which does not receive the solar emanations, should be carried towards that which is exposed to it? And since these particles at each contact only receive a part of the temperature to which they attain, what a prodigious whirlwind must there be supposed to be in the gas!

It appears to me, that the elastic fluids, instead of being bad conductors, possess this property in a high degree, although they probably differ from each other in this respect; and if air which is confined produces effects which seem to prove the contrary, they are owing to some circumstance which modifies this property.

I think it is probable, that this circumstance is the state of compression produced in a gas which cannot acquire a dilatation suitable to the temperature it receives; we have seen that caloric, in combining with the gases, only raises the temperature because the dilatation meets with an obstacle (107); hence it results, that the further the air is removed from the state of dilatation, which it should have, to be in equilibrium of temperature, the greater resistance will it oppose to the combination of the caloric, and the more will it lose of its conducting faculty, so that the air which would take the temperature of the surrounding bodies with facility, if it could acquire suitable dimensions under a given pressure, becomes a worse and worse conductor in proportion as it receives a temperature farther removed from the dimensions which it can take. The air then experiences an effect, which may be compared to that of a body in which the force of cohesion obstructs the action of a liquid, which can effect its solution as soon as this obstruction begins to be diminished.

This explanation is applicable to the conservative property of heat, which Rumford has proved to belong to the air which adheres to particles, such as those of the eider-down;

The slow conducting by confined gases ascribed to some modification.

The gas is probably prevented from acquiring heat, because it cannot expand in that state.

The effect of eider down, &c. is less to prevent the currents, than to oppose the ex-

* Descrip. de l'aérostat de l'Acad. de Dijon.

panfion by its attraction, by which the air adheres to it.

this air only adheres by a true affinity, which probably reduces its dimensions, or at least, opposes its dilatation; and if the water can drive it off, it is only because it combines with these substances, and adheres to their surface by its affinity; so that the air will then experience the same effect from the action of the affinity of the bodies to which it is adherent, as is produced on its elastic effort, by the space within which it is confined, and in which it receives a higher temperature without having the power to dilate.

So that elastic fluids being more alterable in their volume, are also more disposed to give and take heat.

Thus the elastic fluids which dilate much more by a similar change of temperature than liquids and solids, must have the corresponding faculty of entering more easily into combination with caloric: they offer but little resistance to compression; they heat by the reduction of their volume; and they cool when they dilate: do not these effects announce a great disposition to combine with caloric, or to abandon it, and to receive different degrees of saturation from it? and nevertheless, according to the opinion of Rumford, there must be an insurmountable barrier between the most distant temperatures, of the different particles of a gas, when the particles do not meet with a solid body.

The same doctrine applied to liquid water: Sudden accumulation or absorption by its fusion; it conducts better than ice, &c.

It is possible, that liquid substances may be much better calculated to conduct heat than when they are in a solid state; the properties of the reciprocal affinity which produces cohesion, seem to point this out: for since this affinity opposes the dilatation, it will offer an obstacle to the combination of the caloric: this resistance to its introduction is also proved by the quick accumulation which is made of it, as soon as the force of cohesion is destroyed, so that it is opposed to the combination of caloric, as well as to that of other substances; in fact, water seems to take the common temperature more easily, independent of the locomotion of its particles, than ice, which is a very bad conductor, and it is perhaps from this difference, that ice, and all the solids which pass to the liquid state, liquefy at the surface, instead of taking the common temperature.

I only offer these last explanations as conjectures, which may invite to experimental enquiries on a subject which is not indifferent to chemical theory.

XIII.

A Memoir on the Movements which certain Fluids receive from the Contact of other Fluids *. By J. DRAPARNAUP, Curator and Professor of Natural History, at the Medicinal School of Montpellier.

I HAD observed that alcohol attacks, and finishes by even, at length, destroying the calcareous covering of the molluscæ, which are put into it to be preserved. Supposing that this might arise from the alcohol, particularly that which is not well rectified, containing a little acetous acid, I put a little tincture of turnsole into a glass capsule, and poured into it a few drops of alcohol. The tincture did not change colour, but to my great surprise it moved towards the circumference with great vivacity, leaving the bottom of the capsule uncovered: when it had reached the maximum of dispersion, it returned again, and covered the bottom of the vessel which it had abandoned. It is evident, therefore, that missing what I sought, I found that which I did not seek, which frequently occurs in the course of experiments.

Experiment on the action of alcohol which led to the discovery of this impulse.

This curious experiment induced me to make a multitude of others, and to try a great number of substances. At the moment of putting these experiments in order, and of composing this memoir, I recollected having read that *M. Benédict Prevost* had produced this repulsion of water by means of volatile oils, and even of many solid odorant bodies. I therefore consulted the two memoirs which this ingenious philosopher has inserted in the *Annals*, and whose subject is, *The methods of rendering the emanations of odorant bodies sensible to the sight* †. Although my experiments were made with another view, I pass over, in silence, all which are conformable to those of *M. Prevost*, and which I made by employing the same substances: I shall only speak of those which differ from his, either in the results obtained, or in the means employed.

Prevost's memoirs on the emanations of odorant bodies.

1. If a thin stratum of water is put on the bottom of a vessel, and a drop of alcohol is brought to the centre of this

Action of alcohol on water.

* From the *Annales de Chimie*. *Frustrator*, An. XI.

† *Annales de Chimie*. Tom. XXI. et XXIV.

stratum,

stratum, with a glass rod, the water flies in an instant with vivacity, leaving the bottom of the vessel uncovered; when it has reached the maximum of dispersion it returns, and covers again the bottom of the vessel which it had quitted.

Disk of dispersion.

2. I call that part of the vessel which is abandoned by the water the *disk of dispersion*. In the preceding experiment, this disk shows a perfect dryness and all its natural polish.

The repetition of the experiment lessens its effects.

3. The repulsion becomes less considerable, and the disk of dispersion smaller, in proportion as the experiment is repeated in the same water. This arises from the water becoming gradually saturated with alcohol.

Influence of the vessels.

4. The nature of the vessels has no influence on the preceding experiment, nor on those which follow. They take place equally in vessels of porcelain, earthen ware, glass or metal.

5. The form of the vessels has much influence on the second period of the phenomenon, that is to say, on the return of the water, and on the disappearance of the disk of dispersion.

If the vessel is a little concave, the water always comes back, and covers the bottom of the vessel again. It will be obvious, that this is the necessary effect of its gravity.

If the vessel is flat, the water only returns when the disk of dispersion has not attained too great a diameter.

If the bottom of the vessel is a little convex, the water does not return after having been dispersed, and it must be evident, that to do so it would act contrary to the operation of its own gravity.

Motion of the expelled fluid;

6. Being desirous to render the observation of the motions of the expelled fluid easier and more perfect, I substituted tincture of turnsole, which, as is known, is only water coloured by turnsole, for pure water. The results were the same, but much more sensible; and I could then readily distinguish an undulating or trembling motion on the internal edge of the water which surrounds the disk of dispersion; a motion which proves the continual emission of the alcoholic particles against this interior edge, and determines the removal of the water.

and of the impelling fluid.

7. But to complete the proof of the explanation which I have just given of the phenomenon, it was also necessary to render the motions of the impelling fluid sensible: I succeeded by

by an analogous process. I coloured alcohol by means of turnsole; with this substance it takes a very beautiful colour which is not at all similar to the violet blue of tincture of turnsole, but, on the contrary, is of a very vivid blue, analogous to that of indigo, or prussian blue. I then wetted the bottom of a plate with pure water, and, with a glass rod, brought a drop of this coloured alcohol to the centre: the water was driven back with vivacity. In the centre of the disk of dispersion was a blue spot, formed by the coloured alcohol, and the rest of the disk was white like the bottom of the plate. But the proof of the actual continual emission of the coloured alcoholic particles, is that as the water retired, its internal edge, which touched the disk of dispersion, became more and more of a violet colour, analogous to that of tincture of turnsole prepared with water. It is evident, therefore that in natural philosophy, facts are explained by facts, and, that this experiment confirms the consequence I had deduced from the preceding experiment.

8. If the plate is wetted with the alcohol, and a drop of water is put into the centre, the alcohol does not experience any motion; the drop of water flattens, it retains its orbicular form for some moments, at length it finishes by spreading irregularly, mixing with the alcohol, and uniting with it. Water does not
repel alcohol.

9. If the bottom of the plate is covered with a very thin stratum of olive oil, and a drop of alcohol is brought to the centre, the oil is repelled, though more slowly than the water on account of its viscosity, and the bottom of the plate is left dry. Oil is repelled
more slowly than
water.

If the stratum of oil is too thick, it will not quit the bottom of the plate, and the expansive motion of the alcohol takes place only on the superficies of the oil.

10. If a morsel of the fresh rind of lemon or orange is placed in the centre of a wetted plate, the water is sensibly repelled, and the disk of dispersion is agreeably tinged with the prismatic colours, which depends on the disengagement of the essential oil. But this motion has not nearly so much intensity as that produced by means of alcohol. The essential oil
of lemon or
orange rind, pro-
duces repulsion,
but weaker.

11. Convinced by the preceding experiments that every volatile fluid at the atmospheric pressure, was capable of producing this repulsive movement, I employed liquid ammonia. I therefore wetted the plate in the usual manner, and brought The affinity of
ammonia for
water prevented
its repulsive
action,
a drop

a drop of volatile alkali to the centre. What was my surprise to observe, that the slightest motion was not manifested in the water; I however suspected the cause, and believed that this apparent anomaly was owing to the ammonia, which, having a very great affinity with water, combined with it at the instant of their contact.

which was exercised in olive oil.

12. I resolved, therefore, to substitute a fluid to the water, which had less affinity with ammonia, and I chose olive-oil, which I had at hand. I again covered the bottom of the plate with a thin layer of this oil, and brought a drop of the ammonia to the centre: the oil was instantaneously repelled, as it had been by the alcohol.

Ammonia does not repel alcohol:

13. If the plate be wetted with alcohol, and a drop of ammonia is put into the centre, the alcohol is not repelled, the drop of ammonia flattens, and the two liquors evaporate. I thought I perceived a slight tremulation on the edge of the drop of ammonia.

but alcohol repels ammonia.

14. If the plate be wetted with ammonia, and a drop of alcohol is brought to the centre, the ammonia is repulsed like pure water. It appears, therefore, from this experiment, and the preceding, that the horizontal or lateral force of expansion of the alcohol is superior to that of the ammonia.

Olive-oil and water have no repulsive action.

15. A drop of olive oil put into a wetted plate, did not produce any motion in the water. It was the same with water beat up with oil to the consistence of an unguent. *M. Præst*, in his memoirs, seems to announce results obtained with the fixed oils, which are contrary to my experiments, and particularly to these.

I shall not enlarge farther on experiments which some may, perhaps, think more curious than useful. But when the attractive powers of yellow amber, or of the loadstone were first observed, neither their importance, nor the astonishing discoveries to which they have since led, were suspected.

XIV.

Letter from Mr. CUTHBERTSON respecting his Galvanic and Electrical Experiments.

To Mr. NICHOLSON.

DEAR SIR,

IN consequence of the note which you have been so obliging ^{Introduction.} as to add to my letter, addressed to Dr. Pearson, inserted in your Journal for this month, I have to say, that *the troughs were used collaterally*: I shall now be happy to see your remarks. It appears, that I ought to have been more explicit, and therefore, I beg leave to offer the following additional observations *.

The two last mentioned experiments in the letter alluded to, were compared with common electrical discharges, with a view to prove what quantity of coated glass would be requisite to ignite the same lengths of wire.

Two jars, each containing about 170 square inches of coated ^{Experiment.} surface, were set to the conductor of a 24 inch single plate ^{D. flagration of wire by jars.} electrical machine, with my universal electrometer loaded with 31 grains, (see Quarto Journal, Plate XXII. Vol. II.)

* I ought certainly to have mentioned the arrangement of the troughs, and likewise I ought not to have said so vaguely, that double quantities of galvanic fluid, only burn double lengths of wire, because I am strongly of opinion, that the reason why galvanic discharges from troughs do not act upon metals in the same ratio as common electric discharges do, proceeds from some defect in the arrangement, and also construction.

I find in my notes of improvements for the 6th of June, 1803, ^{Pile of large plates.} that I had made a pile of 16 pairs of plates of 10 inches diameter, and that eight of them laid upon each other in the usual manner, with cloths wetted with diluted muriatic acid, burned one inch of wire of 1-195th part of an inch in diameter, and that 16 pairs burned four inches of the same wire. This experiment was repeated on the 8th of June, with the same result, with respect to metals, but gave strong and loud sparks from metal to metal, ^{very loud galvanic sparks.} sufficient to be heard at 300 yards distance, which result, I believe, has never been obtained from troughs, so as to be heard, indeed, at any distance. For the last experiment, the cloths were wetted in a strong solution of muriate of ammonia.

Eight

Eight inches of the same sort of wire were laid in the circuit, 57 revolutions of the plate caused the electrometer to discharge the jars which ignited the wire perfectly, as in the ninth experiment. Then six inches of the wire were laid in the circuit, and the above number of revolutions caused the discharge, the wire being deslaggerated and fused into balls in the same manner as in the eighth experiment.

Deduction as to the quantities of electricity in a jar and a pile.

Probability that Mr. Wilkinson's wire was thinner than stated.

Hence I conclude, that 340 square inches of coated glass properly constructed, will bear a charge equal to a galvanic battery of 1080 square inches of surface.

The result of the above experiments gives me reason to think, that there is a mistake respecting the diameter of the wire ignited by Mr. Wilkinson's batteries, as mentioned in your Journal, Vol. VII. p. 297, to which you refer, because to ignite one half inch of steel wire of *one seventieth of an inch in diameter*, will require a power sufficient to ignite 120 inches of wire $\frac{1}{175}$ part of an inch in diameter, by common electrical discharges, which is a power equal to two of my common electrical batteries, (see your Quarto Journal, Vol. II. p. 523.)

The greatest power of 60 pairs of 6 inches square plates that ever has been known, was that of igniting 16 inches of wire of $\frac{1}{175}$ part of an inch in diameter. Mr. Wilkinson's trough of 100 pairs of plates of 4 inches square is of much less surface, and as he says, it is a less favourable size, from which, and from the above experiments, I conclude, that such a battery has not the power of igniting one half an inch of wire of *one seventieth of an inch in diameter*, unless galvanic discharges act upon metals in some manner different to common electrical discharges, but with which I am unacquainted; perhaps Mr. Wilkinson will be kind enough to clear up this remark.

I am, with due respect,

Dear Sir, Your very humble Servant,

JOHN CUTHBERTSON.

Poland Street, Soho,
June 19, 1804.

XV.

Chemical Examination of the Ochroites, a Mineral not hitherto well known, containing a New Earth. By KLAPROTH.*

THE fossil which forms the subject of the present analysis, and to which I have given the name of ochroites, for reasons to be stated hereafter, is found in the mine of Bafnætes, near Riddarhytta in Westmannland. History of the fossil.

The first account of this mineral we owe to Cronstedt, who furnished a description of it, together with that of another mineral, found at Bispsberg in Delecarlien†. Scheele considered both as species of iron ores, and gives to them the name of lapis ponderosus, ponderous stone, (schwerstein) or tungsten, which he describes in his mineralogy as *terrum calciforme, terra quadam incognita intime mixtum*‡. He likewise examined this tungsten, and made us acquainted with its true nature. The mineral which he examined, was, however, the pearl-coloured tungsten of Bispsberg, and from this he concluded, that the examination of the tungsten of Riddarhytta was necessary, considering it a mineral of the same nature, he distinguished it by the name of *reddish tungsten*. First noticed by Cronstedt; and examined by Scheele

Soon afterwards D'Elhuyar analysed both minerals, he verified the analysis of the true tungsten, but proved that the conjecture of Scheele concerning the other mineral was founded in error; the results of his analysis showed that the fossil known by the name of reddish tungsten, was composed of 54 parts of lime, 24 of iron, and 22 siliceous earth. From what follows, it will, however, become obvious, that this mineral contains neither lime nor tungsten, but a new earth hitherto unknown.

External Characters of the Ochroites.

The colour of this mineral is between carmoisin red, clove-brown, and reddish-brown. It is compact, breaks, External characters of Ochroites.

* Gehlen's new Journal of Chemistry, Vol. II. part. iii. p. 303.

† Transactions of the Swedish Academy of Sciences, 1751. p. 235.

‡ And also in Cronstedt's Mineralogy by Magellen, Vol. I. p. 46.
ing

ing in irregular, not very sharp or angular pieces. It is perfectly opaque. Its powder is reddish-gray. It is not very hard, but brittle, and very ponderous.

Its specific gravity is 4,660. Cronstedt states it to be = 4,988.

A.

Analysis of the
Ochroites.

a. A piece of the mineral after having been ignited to redness, lost 2 per cent. Its reddish colour had been changed to brown. Its figure had suffered no alteration.

b. One hundred grains of the finely levigated mineral ignited for half an hour, lost five grains. Its colour was changed to a dark brown.

B.

a. One hundred grains of ochroit, after being mixt with 200 grains of carbonate of potash, were strongly ignited, the mass which could not be rendered fluid, was reddish, grey and brittle. On being diffused through water, as usual, the obtained solution was colourless. It remained perfectly transparent; a proof that it did not contain tungsten oxide; nitrate of silver, mercury, lead, barytes, &c. proved the absence of acids.

b. The insoluble residue of the last process was boiled in nitro-muriatic acid, the siliceous earth being separated, the solution was decomposed by potash, and the whole boiled for some time. The alkaline fluid after being neutralized with muriatic acid, and then mingled with carbonate of potash, suffered no change.

C.

a. 200 grains of the finely pulverized mineral, were first boiled in two ounces of muriatic acid, to which half an ounce of nitric acid was gradually added, and the digestion continued for some time. The whole became thus dissolved except the silix contained in the mineral. Its quantity amounted to 68 grains.

b. To the solution obtained in the last process, carbonate of ammonia was added so long, till no permanent precipitate was produced. On letting fall into it seccinate of ammonia, a curdly precipitate fell, which vanished again on agitation, leaving

leaving merely a pale red precipitate of succinate of iron, this being collected, washed, dried, and strongly ignited, yielded nine grains of oxide of iron.

c. The fluid thus freed from iron, and now colourless, was decomposed by carbonate of ammonia. The precipitate obtained was white, and weighed 168 grains, on being deprived of water and carbonic acid by heat, its white colour changed to cinnamon-brown. It weighed 109 grains.

d. All the water employed for washing the different precipitates were mingled, evaporated to dryness, and the ammoniacal salt volatilized; a minute quantity of a muriate was obtained, the basis of which could not be determined.

From what follows it will become evident, that the cinnamon-brown precipitate (c.) which forms the principal part of the fossil is a peculiar earth, distinct from all the others hitherto known. The characteristic property which it possesses of acquiring a light-brown colour after being heated, has induced me to call it ochroit earth*, which may also serve for the mineral itself.

According to this analysis, 100 parts of the ochroite of Ridderhytta contain,

Ochroit earth	-	-	-	54.50
Silex	-	-	-	34.
Oxide of iron	-	-	-	4
Water, &c. (A. b.)	-	-	-	5
Loss	-	-	-	2

100.

Characteristic Properties of Ochroit Earth.

1. Ochroit earth is capable of combining with carbonic acid during its precipitation from acids by carbonated alkalies, and strongly consolidating a portion of water.

Ochroit earth,
1. combines
with carbonic
acid.

100 grains of the earth precipitated by carbonate of ammonia, and strongly dried, lost on being neutralized by nitric acid, 23 grains: 100 grains of the same earth lost after being strongly ignited, 35 grains, 100 parts of carbonate ochroite therefore consists of

Ochroit earth	-	-	-	65
Carbonic acid	-	-	-	23
Water	-	-	-	12

100.

* From the Greek word *οχροεις*, (Hæfescens,) brownish yellow.

1. It is brown.

2. Ochroit earth, after being freed from carbonic acid and water, by heat, always appears in the form of a cinnamon-brown powder. The intensity of the colour is in proportion to the heat applied. This colour is not owing to the presence of iron, or manganese, &c. but it is a characteristic property of the earth.
3. Not reducible.

3. Ochroit earth included in a charcoal crucible, and exposed to the heat of the porcelain furnace, suffered no change whatever.
4. Not fusible as the microcosmic salt nor borax.

4. Urged by the blow-pipe, it becomes phosphorescent; fused with phosphate of soda and ammonia, it becomes tinged by it, without effecting a solution of the earth. The salt acquires merely a marbled lemon yellow colour. Borax has likewise no chemical effect upon it. This salt only effects a mechanical division. The earth always appears diffused through the borax in minute *floculi*.
5. Gives an uneven brown as a porcelain colour.

5. Ochroit earth mixed in different proportions with proper fluxes, and applied for painting of porcelain, proved unsuccessful. The painted articles were light brown, but the colour was not uniform; a proof that no combination had been effected.
6. Difficultly soluble in acid if the earth be pure; but easily if carbonated.

6. Ochroit earth combined with carbonic acid is easily soluble with effervescence in acids. The taste of the solution is very rough and astringent. The concentrated solution is of an amethyst red colour; diluted with water, it becomes colourless. Ignited ochroit earth, on the contrary, is difficultly soluble in acids in the cold; if nitric acid be employed, the solution is yellowish red.
- Nitr'c acid.

7. The combination of ochroit earth with sulphuric acid, is crystallizable. The figure of the crystals formed in the mass of the fluids is the octahedron. They are heavy, of a pale amethyst colour, and difficultly soluble in water; but the sulphate of ochroit with excess of acid, is more soluble; the figure of the crystals formed on the sides of the vessel, are needle-shaped, radiating from a centre. They are more soluble than the former.
7. Sulphate of ochroit is crystallizable, and pale amethyst colour.

8. If a solution of sulphate of soda be mingled with a solution of muriate or nitrate of ochroit, a mutual decomposition takes place. A white insoluble precipitate is formed, consisting of sulphuric acid united to the ochroit earth. This combination
- Sulphate of soda decomposes muriate or nitrate of ochroites.

The insoluble sulphate of ochroit,

tion may be decomposed by boiling it with double its weight of carbonate of soda. By this means ochroit earth may be obtained very pure.

9. Ochroit earth is likewise soluble in sulphureous acid, the solution crystallizes in needles of a pale amethyst colour.

10. Muriatic acid dissolves ochroit earth, and yields crystals, the figure of which is the prism. It is soluble in alcohol without imparting to its flame any particular colour.

12. Acetite of ochroit could not be crystallized, but yielded an adhesive mass.

13. Nitrate and muriate of ochroit is decomposable by carbonated earths and alcalies, the precipitate is milk-white. Alcalies and earths freed from carbonic acid, occasion a yellowish grey precipitate.

14. Prussiate of potash precipitates ochroit from all its neutral solutions, milk white. The precipitate is soluble in muriatic and nitric acid.*

15. Tincture of galls occasions no change in the solutions of this earth.

16. Hidroguiretted-hidro sulphurett of ammonia precipitates the solution of ochroit earth, yellowish white.

17. Water impregnated with sulphuretted hydrogen occasions no change in the solutions of ochroit earth.

18. Succinates precipitate ochroit earth white.

19. Phosphate of soda, occasions in the solutions of this earth a white precipitate, which again vanishes by the addition of nitric or muriatic acid.

20. Tartrites of potash also precipitate this earth white.

21. Oxalates effect a like decomposition, the oxalate of ochroit, however, is not soluble in nitric or muriatic acids.

22. Alcalies and alkaline carbonates do not act on ochroit earth.

23. Ammonia feebly acts on it, under certain circumstances, as may be evinced from the following experiment :

A solution of nitrate of ochroit, prepared by dissolving 100 grains of carbonate of ochroit (not absolutely free from iron,) in nitric acid, was decomposed by carbonate of ammonia, and digested in the fluid, containing a considerable quantity of carbonate of ammonia in excess, for some days. The fluid

may be decomposed by boiling with carb. of soda; and the earth obtained pure.

9. Sulphureous acid dissolves ochroit earth; crystallizable. Muriatic acid also dissolves it, and crystallizes. The alcohol solution does not colour flame.

Acetite of ochroit not crystallizable.

The nitrate and muriate precipitable by earths and alkalis, and by prussiate of potash; white.

Not by galls,

but by hidro. hidro. sulph. of ammonia.

Not by hidro. sulph. water,

but by succinates.

Phosphate of soda precipitates, &c.

Tartrites precipitate it, and oxalates.

Alcalies do not act on the earths

Ammonia feebly.

* If the earth contained the muriates and quality of iron, it becomes by this means manifested.

which had acquired a yellow colour, was separated and neutralized by sulphuric acid, and then placed in a warm place. A grey precipitate was thus obtained, which on being dried, weighed $1\frac{1}{4}$ grains. This precipitate, after being dissolved in nitric acid, yielded a blue precipitate by prussiate of potash; this being separated, a white flocculent precipitate fell down by dropping into the remaining fluid carbonate of potash. This method is therefore applicable for separating a minute quantity of iron, that may be contained in the fluid.

General remarks
and characters
of the ochroit
earth.

From what has been stated, it becomes obvious, that the ochroit earth bears the nearest relation to yttria, for like this it forms a connecting link between the earths and the metallic oxides. Like yttria it has the property of forming a reddish coloured salt with sulphuric acid, and is precipitable by prussiate of potash, but it differs from yttria, that it does not form sweet salts, that it is not (at least very sparingly) soluble in carbonate of ammonia, and that when ignited it acquired a cinnamon brown colour. It farther differs from yttria by not being soluble in borax or phosphate of soda when urged upon charcoal before the blow-pipe, which salts easily effect a solution of yttria, and melt with it also into a pellucid pearl.

XVI.

*Letter from VAN MARUM to J. C. Delam  therie, on Ritter's Galvanic Experiments.**

New experiment
in proof of the
identity of elec-
tricity and gal-
vanism.

MR. ORSTED of Copenhagen, on his way through Harlem, having shewn me, by means of the apparatus in Teyler's Museum, some discoveries of his friend Ritter of Jena;† I made at the same time a new experiment with him, which affords a fresh proof of the identity of the fluid set in motion by Volta's pile and the common electrical machine, and an account of which I imagine will be acceptable for your Journal.

Two wires of
platina, kept 5
minutes in the
chain of commu-
nication be-
tween the two
poles of the pile,
convulsed the
legs of a frog,

Mr. Orsted having shown me, that two wires of platina, after having been kept five minutes in the chain of communication between the two extremities of a galvanic pile, acquired

* *Journ. de Physique*, December, 1803, Vol. LVII. p. 471.

†. See Ritter's experiments, as communicated by Dr. Orsted, in this present and some other numbers of the Journal.

thereby

thereby the faculty of throwing the legs and thighs of a frog into convulsive movements, according to the discovery of Mr. Ritter; I proposed to him, to pass the stream of fluid from a powerful electrical machine through these same wires of platina, in order to observe, whether it would have the same effect on them as the stream from the galvanic pile. We employed the plate machine of 35 inches diameter, and of the new construction, described in the *Journal de Physique* for June, 1791, vol. 38, (or Philosophical Journal, quarto series, No. 2.)* Having passed the stream from it through the wires of platina, held a quarter of an inch from the conductor, for the space of five minutes, we touched with them the crural nerves of a frog prepared in the usual manner, and observed immediately the same convulsive movements in its legs and thighs, though not so strong as in the preceding experiment. This less powerful effect of the stream from the machine perfectly answered my expectations, since my experiments in November, 1801, described in my letter to Mr. Volta, shewed me that the stream from the machine had not more than five sixths of the velocity or strength of the stream set in motion by a similar pile.

We then repeated the experiment, keeping the wires of platina in contact with the conductor of the machine, while we passed the stream through them. Then holding the wires one in each hand, as in the preceding experiment, and in contact with the two crural nerves, but keeping their opposite extremities separate, their effect on the same frog, the sensibility of which was greatly weakened, was scarcely perceptible: but on bringing the upper extremities of the wires together, while the lower remained in contact with the crural nerves, we noticed very striking convulsions in the legs of the frog, every time the extremities were made to touch. A little sealing wax, which had been used to keep the wires of platina insulated when held to the conductor, and which still adhered to one of the upper ends, rendered the experiment still more evident; for when we brought these ends together in such a manner, that a little of the wax prevented them from being in perfect contact, the legs of the frogs exhibited none of the

and exposed to the stream of a plate machine for a like time did the same, but more weakly.

The experiment was repeated, the wires having been kept in contact with the conductor; when the sensibility of the frog being much weakened, the effect was scarcely perceptible, unless the upper extremities of the wires were brought into contact. When a little sealing wax prevented the absolute contact of the wires, no effect took place.

* It is a machine, that produces positive and negative electricity, like the pile of Volta. *Delamétherie*.

Herschel's discovery of invisible solar rays has given rise to another.

Our knowledge of light at a stand since Newton;

Still Herschel found, that invisible rays, beyond the spectrum, raised the thermometer. Ritter, repeating the experiment, conjectured, that light contained invisible rays acting chemically likewise; and found, that muriate of silver, exposed to the spectrum, grew blackest beyond its violet edge, and the effect diminished in pro-

THE important discovery of invisible solar rays Herschel has enriched natural philosophy, has made to another little known, even in the country where made.

Our knowledge of light had made no perceptment since Newton, when Herschel found, that phenomena occurring during the decomposition of the prism had not yet been noticed. Philosophers contented themselves with perceiving different colours, examining by other processes, whether phenomena visible to the eyes did not take place. Herschel, the thermometer, discovered invisible rays external to the spectrum, that possess the property of raising the thermometer. Mr. Ritter repeated his experiments with success, considering, that the different rays of light produce chemical changes in bodies susceptible of them, that light likewise contained invisible rays, which act chemically. He exposed muriate of silver to the solar spectrum, and found his conjecture substantiated. Muriate of silver very soon became black beyond the violet edge of the spectrum; blackened a little less in the blue, and more and more in proportion to the distance from the violet edge, till it became null. On exposing muriate of silver to the spectrum, that is to say, a little disorganised

The solar spectrum therefore is accompanied by two invisible rays, one on the red side, which favours oxygenation, and the other on the violet side, which favours disoxygenation. The visible and coloured rays partake more or less of the properties of these invisible principles, whence we ought to infer, that all the coloured rays contain more or less of these principles.

These experiments succeed very well with phosphorus. On letting the invisible ray adjoining the red fall on it, it immediately emits white fumes: but if the invisible ray adjoining the violet be thrown on the phosphorus in a state of oxygenation, it is instantly extinguished, with the same rapidity as a frog is convulsed in galvanic experiments.

These experiments readily accord with some others made by the same gentleman. He kept one of his eyes for some minutes in contact with the negative conductor of Volta's pile, and after this operation all objects appeared red to him: but after having kept it in contact with the positive conductor, he saw every thing blue. It is to be observed here, that the retina and optic nerve, when the external part of the eye is brought into the negative state, become positive, and *vice versa*; because the eye is filled with a fluid, in which the same distribution of electricity must take place, as in water and other fluids. It is in the positive state therefore, that the optic nerve perceives all objects of a red colour; and in the negative state they appear violet. The chemical action of positive electricity likewise is the same as that of red light; that is, they both favour oxygenation. Negative electricity and the violet ray possess the same analogy, both promoting disoxygenation: as the experiments with the galvanic pile have sufficiently shown.

If I might be allowed to add any observation to these important discoveries, I would mention one of the most commonly known facts; that the opposite electricities, when united, produce light; which seems to demonstrate synthetically, what the preceding experiments have shewn by analysis.

This account was read to the philomathic society several months ago, since which time Mr. Ritter has published some new observations that deserve notice. He has found, with all the prisms he used, that the solar rays give two coloured spectrums, which spread in proportion to their distance from the prism, so that at a certain distance one nearly covers the other.

invisible ray beyond the red. Thus there is an invisible ray promoting oxygenation, and another the contrary, and the visible and coloured rays partake more or less of the properties of both.

The invisible ray on the red side likewise kindles phosphorus; while that on the violet side extinguishes it instantly.

One of the eyes being kept some minutes in contact with the negative conductor of Volta's pile, all objects appeared red to it; in contact with the positive blue.

In the former case the optic nerve is in the positive state; in the latter, negative.

The chemical action of positive electricity likewise favours oxygenation, like red light; and negative, the contrary.

The two electricities too produce light by their union; which seems to prove synthetically their identity with the chemical rays.

Mr. R. has since found, that the prism affords the two spectrums.

The

distinct at a little distance, as 4 inches, but confounded together when farther off.

And the chemical rays may be completely separated from the coloured, without destroying them.

So that we may produce a coloured spectrum devoid of chemical action.

The calorific rays have not been proved to be separable from the chemical; but it may be presumed they are, since the former differ in degree in winter and summer, the latter do not.

That they accompany each other, though separable, no greater difficulty than attends electricity.

The experiment must be made at a very small distance from the prism, that of four inches for example, to distinguish the two spectrums, which become confounded together in proportion to their distance from the prism. No doubt this is the reason why the phenomenon has so long escaped the notice of philosophers. This observation was accompanied by another, still more important; namely, that the chemical rays may be completely separated from the coloured rays. If the invisible rays of the violet side be made to fall on the red part of the solar spectrum, the process of oxidation may be completely suspended there, and even disoxidation produced, without destroying the red colour; and by means of several prisms we may even separate all the coloured rays from the chemical rays. We may thus produce a coloured spectrum devoid of chemical action; and a series of chemical rays, analogous to the spectrum, without any mixture of coloured rays.

We have not yet any accurate experiments, to determine whether the calorific rays be likewise separable from the others, particularly from the chemical rays: but the comparison of different experiments made in winter and summer, when the degrees of heat are different, though no difference in the force of the chemical rays at different seasons has been observed, lead to a belief that the calorific rays are separable from the chemical.

It may be asked, why the different rays found in light frequently accompany each other, though they can subsist separately: and this question no doubt may be answered, when we are able to say, why the different functions of electricity accompany each other, though they likewise are separable.

XVIII.

*On Spontaneous Inflammations. By G. C. BARTHOLDI, Professor of Physic and Chemistry.**

Definition of spontaneous inflammation.

SPONTANEOUS inflammation is that which is manifested in a combustible body, without the immediate contact of any ignited matter.

* From the *Annales de Chimie*, Vol. XLVIII. p. 249, or No. 144.

These

These kinds of combustion may be occasioned by different Causes. causes, the principal of which are:

1. Considerable friction.
2. The action of the sun.
3. The disengagement of caloric, produced in bodies, which, though not combustible, by being brought near to other combustible bodies, may communicate such a degree of heat to them, that they inflame by the contact of the air.
4. The fermentation of animal and vegetable substances heaped up in a large mass, which are neither entirely dry, nor too wet, such as hay, dung, &c.
5. The accumulation of wool, cotton, and other animal and vegetable substances, covered with oily matter, particularly drying oil.
6. The preparation of linseed oil for printers' ink, of varnishes, and in general of all fat.
7. The torrefaction of different vegetable substances.
8. The sulphurated and phosphorated hydrogen gases which are disengaged in many of the operations of nature, and of which, the latter generally inflames by the sole contact of atmospheric air, even at a low temperature, and which is often seen at the surface of the earth like a small flame, known by the name of Jack o'Lantern, in places in which there are animal substances in a state of putrefaction: if other combustibles are met with where the disengagement takes place, they readily catch fire.
9. The phosphuret of lime and potash, which may be formed in the preparation of charcoal, particularly in that from turf, and from some sorts of wood which grow in marshy situations. This charcoal by being wetted, or by simply attracting the humidity of the air, forms phosphureted hydrogen gas, which, by the contact of the atmospheric air inflames, and may set fire to the whole mass of charcoal.
10. The phosphorus which is sometimes, though rarely, formed in the carbonisation of different sorts of wood, without combining either with lime or with potash in the state of phosphuret. These charcoals do not inflame spontaneously at the common temperature of the atmosphere, but they produce a detonation by percussion with nitrate of potash, or with some other nitrates and metallic oxides to which the oxygen adheres but feebly, and which, being in the state of thermoxide, retain much latent caloric.

I. *Friction.**Friction.*

It is generally known that by rubbing two bodies against each other, they are heated; the intensity of the produced heat depends on several circumstances, and chiefly varies in proportion to the hardness of the friction, to the nature, and to the surface of the rubbing bodies: if the friction takes place between combustible bodies, such as the woods, the heat which it excites is frequently sufficient to inflame them: if the bodies are not combustible, such as stones, or but little combustible, like the metals, they do not themselves inflame, but they can communicate such a degree of heat to other combustibles which surround them, that these can inflame by the contact of atmospheric air.

Dr. Palcani having repeated the experiments which have been long known for obtaining fire by the friction of two pieces of wood, to one of which he gave the form of a tablet, and to the other that of a spindle or a cylinder, he has allowed me to give the results of some of his experiments here, to shew that more attention should be paid to the choice of the woods which are destined to rub against each other, in the construction of machines and instruments.

Experiments
with a tablet
between two
cylinders,

Two cylinders of	Tablet.	Duration.	Effect.
Box-wood rubbed against	Box-wood ^{during}	5 Minutes	Sensible heat
<i>Idem</i>	Poplar	<i>Idem</i>	<i>Idem</i>
<i>Idem</i>	Oak	<i>Idem</i>	<i>Idem</i>
<i>Idem</i>	Mulberry	3 Minutes	Cons. heat and smoke
<i>Idem</i>	Laurel	<i>Idem</i>	<i>Idem</i>
Laurel	Poplar	2 Minutes	<i>Idem</i>
<i>Idem</i>	Ivy	<i>Idem</i>	<i>Idem</i>
Ivy	Box-wood	3 Minutes	<i>Idem</i>
<i>Idem</i>	Hazle	<i>Idem</i>	<i>Idem</i>
Olive	Olive	<i>Idem</i>	<i>Idem</i>
Mulberry	Laurel	2 Minutes	Con. he. sm. & black
Ash	Oak	5 Minutes	Sensible heat
<i>Idem</i>	Fir	<i>Idem</i>	<i>Idem</i>
Pear-tree	Oak	<i>Idem</i>	<i>Idem</i>
Cherry-tree	Elm	<i>Idem</i>	<i>Idem</i>
Plum-tree	Apple-tree	<i>Idem</i>	<i>Idem</i>
Oak	Fir	<i>Idem</i>	<i>Idem</i>

On changing the experiment, and rubbing a cylinder of ^{and with a cy-} one of these woods between two tablets of the other, for ex- ^{linder between} ample, a cylinder of poplar between two tablets of mulberry, ^{two tablets.} the augmentation of the rubbed surfaces which are in contact with the air, produced a much more considerable heat, and nearly all the woods mentioned above took fire.

The effect of the friction also varies according as the woods ^{The direction of} employed, particularly if they are of the same species, are ^{the grain of the} rubbed in the direction of the grain of the wood, or when the ^{wood affects the} grains of the woods cross each other. In the first case the ^{friction.} friction and the heat are much more considerable than in the second.

In large machines, in which there is a great deal of friction, ^{Preventives} the heating is prevented by directing a continual current of ^{against sponta-} cold water on the rubbing surfaces: in common machines, ^{neous inflamma-} and in coaches, waggons, &c. it is diminished by covering the ^{tion by friction.} rubbing surfaces with some oily matter. There have been many examples, during the great heat of summer, of coaches, and other machines subjected to a rapid motion, having taken fire, because the greasing them had been neglected. The grease, by hardening on the rubbing surfaces, instead of diminishing the friction, increases it; and as this covering is very combustible, it renders spontaneous inflammation still more easy. It is also preferable, in many circumstances, to rub machines with soap, talc, plumbago, or other substances, which, without being oily, are very unctuous to the touch.

2. Action of the Sun.

By concentrating the solar rays with convex glasses or con- ^{Action of the} cave mirrors, the strongest heat is produced, all sorts of ^{sun.} combustibles are set on fire, and the most refractory substances are melted: it may happen that other bodies may be found in circumstances, in which, without our concurrence, they produce the effects of glasses and burning mirrors: although these effects are rather physical than chemical, it is nevertheless essential to make them known, to guard against their danger. There are examples of fires produced by large glass bottles, filled with water and exposed to the sun, in an apartment. Whenever the form of the vessel is nearly similar to that of a lenticular

lenticular or spherical glass, the rays are refracted, and by uniting in a focus, produce a heat capable of setting fire to the combustible bodies within it.

3. *The Heat excited in Bodies not combustible.*

Heat from in-
combustible
bodies.

It is known that quick-lime plunged into water, or simply moistened, produces a considerable heat. This method has even been employed with success for heating apartments, green-houses, hot-beds, &c. at little expence. This property of quick-lime of disengaging much heat by the contact of water, and that, not less dangerous, of dissolving or corroding animal substances immersed in it, require the greatest precautions when a considerable quantity of quick-lime is kept together. To preserve it, it must be protected from the contact of the air and of every species of humidity, and it must be carefully kept at a distance from all combustible bodies, such as wood, hay, straw, &c. which might inflame spontaneously, if the lime contracted the least humidity. The *Journal de la Haute-Saône* gave an account, last year, of the destruction of a barn, one of the wooden partitions of which took fire from a heap of quick-lime, intended for the repairs of the farm, having been carelessly laid against it.

In nature a great number of analogous phenomena occur, in which bodies, by changing their composition, or by contracting new combinations, heat so much, or disengage such a quantity of caloric, that other combustibles which are near them may take fire.

(*To be continued.*)

XIX.

Discovery of two new Metals in crude Platina. By SMITHSON
TENNANT, Esq. F. R. S.

New metal
Iridium.

AT the last meeting of the Royal Society a paper of Mr. Tennant was read, on the analysis of the black powder which remains after dissolving platina, shewing that it contains two new metals. Mr. Tennant's first experiments were made last summer,

summer, and had been communicated to Sir Joseph Banks, after which an account of one of these metals appeared in France, by M. Descotil *, and also by M. Vauquelin. The properties ascribed to it by the French chemists are, 1. That it reddens the precipitates of platina made by sal ammoniac; 2. That it dissolves in marine acid; 3. That it is precipitated by galls and prussiate of potash. The properties mentioned by Mr. Tennant are, that it dissolves in all the acids, but least in marine acid, with which it forms octahedral crystals. The solution with much oxygen is deep red, with a smaller proportion green or deep blue. It is partially precipitated by the three alkalies when pure. All the metals, excepting gold and platina, precipitate it. Galls and precipitate of potash take away the colour of this solution, but without any precipitate, and afford an easy test of its presence. The oxide therefore loses its oxygen, by water alone. When combined with gold or silver, it cannot be separated by the usual process of refining these metals. As the French chemists have not given a name to the metal, Mr. Tennant inclines to call it *Iridium*, from the various colours of it in solution.

The second new metal is obtained by heating the black powder with pure alkali in a silver crucible. The oxide of this metal unites with the alkali, and may be expelled by an acid and obtained by distillation, being very volatile. The oxide has a very strong smell, from which Mr. Tennant has called it *Osmium*. It does not redden vegetable blues, but stains the skin of a deep red or black. The oxide in solution with water has no colour, but by combining with alkali or lime becomes yellow. With galls it gives a very vivid blue colour. All the metals, excepting gold and platina, precipitate this metal. If mercury is agitated with the aqueous solution of the oxide, an amalgum is formed, which, by heat, loses the mercury, and leaves the osmium pure as a black powder.

* See our Journal, Vol. VIII. p. 118.

SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

Figure of the Orbits of the new Planets. By JEROME DE LALANDE.

Figure of the orbits of the new planets.

THE mean distance of each of these planets from the sun is 2,77, which answers to 227 million of geographical miles nearly.

Piazzi or Ceres, discovered Jan. 1, 1801.

Revolution	4 years, 7 months, 10 days.	
Mean longitude, Jan. 1, 1804	-	10 ^h 11 ^m 59 ^s
Annual motion	-	2 18 14
Aphelion	-	10 26 44
Node	-	2 21 6
Equation of the orbit	-	9 3
Eccentricity	-	0', 079
Inclination	-	10 37

Albers or Pallas, discovered March 28, 1802.

Revolution	4 years, 7 months, 11 days.	
Mean Longitude, Jan. 1, 1804	-	9 ^h 29 ^m 53 ^s
Annual motion	-	2 18 11
Aphelion	-	10 1 7
Node	-	5 22 28
Equation of the orbit	-	28 25
Eccentricity	-	0,2463
Inclination	-	34 39

Philosophical Transactions of the Royal Society of London, for the Year 1804. Part I. 4to, 182 Pages, with five Plates and 26 Pages of Metereological Journal.

Philosophical Transactions of the Royal Society.

THIS Part contains—1. The Bakerian Lecture, Experiments and Calculations relative to Physical Optics; by Thomas Young, M. D. F. R. S. 2. Continuation of an Account of a peculiar Arrangement in the Arteries distributed on the Muscles of slow moving Animals, &c.; in a Letter from Mr. Anthony Carlisle to John Symmons, Esq. F. R. S.

3. An

3. An Account of a curious Phenomenon observed on the Philosophical
Glaciers of Chamouny; together with some occasional Obser- Transactions of
vations concerning the Propagation of Heat in Fluids; by the Royal So-
ciety.
Benjamin Count Rumford, V. P. R. S. Foreign Associate of
the National Institute of France, &c. &c. 4. Description of
a triple Sulphuret of Lead, Antimony, and Copper, from
Cornwall; with some Observations upon the various Modes
of Attraction which influence the Formation of Mineral Sub-
stances, and upon the different Kinds of Sulphuret of Copper;
by the Count de Bournon, F. R. S. and L. S. 5. Analysis
of a triple Sulphuret of Lead, Antimony, and Copper, from
Cornwall, by Charles Hatchett, Esq. F. R. S. 6. Observa-
tions on the Orifices found in certain poisonous Snakes, situ-
ated between the Nostril and the Eye; by Patrick Russell,
M. D. F. R. S.: with some Remarks on the Structure of
those Orifices, and the Description of a Bag connected with
the Eye met with in the same Snakes; by Everard Home,
Esq. F. R. S. 7. An Enquiry concerning the Nature of
Heat, and the Mode of its Communication; by Benjamin
Count Rumford, V. P. F. R. S. Foreign Associate of the
National Institute of France, &c. 8. Experiments and Ob-
servations on the Motion of the Sap in Trees; in a Letter
from Thomas Andrew Knight, Esq. to the Right Hon. Sir
Joseph Banks, Bart. K. B. P. R. S. Appendix, Metereolo-
gical Journal kept at the Apartments of the Royal Society, by
Order of the President and Council.

*Analytical Essays towards promoting the Chemical Knowledge of
Mineral Substances.* By MARTIN HENRY KLAPROTH,
Professor of Chemistry, Assessor to the Royal College of Phy-
sicians, Member of the Royal Academy of Sciences at Berlin,
and various other learned Societies. Vol. II. 8vo, 267 Pages.
Translated from the German. Cadell and Davies.

THIS work is translated by the same learned chemist to Analytical
whom the scientific world is obliged for the former volume. Essays.
Its valuable contents are as follow. 73. Examination of the
Auriferous Ores from Transylvania. 74. Analysis of the
sulphated Oxyd of Manganese from Transylvania. 75. Exa-
mination of Tungstate of Lime (Scheelium). 76. Gadolinite.
77. Examination of the Egyptian Natrum (Soda). 78. Stri-
ated

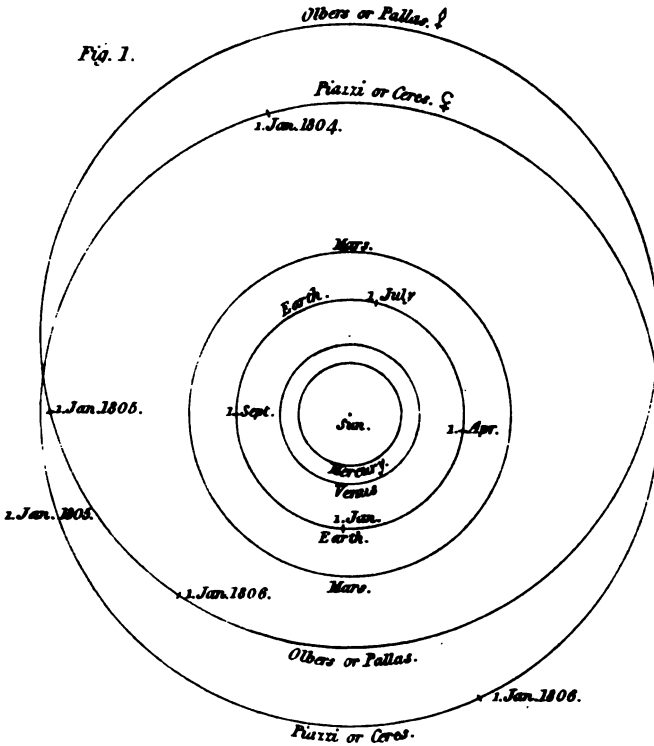
Analytical
Essays.

ated Soda. 79. Analysis of the native Muriate of Ammoniac. 80. Examination of Saffolin. 81. Examination of the Plumose Alum from Freyenwalde. 82. Capillary Salt (*Halotrichium*) from Idria. 83. Elastic Bitumen, from Derbyshire. 84. Examination of Mellilite. 85. Umbra (Umbel). 86. Examination of the muriated Lead Ore. 87. Phosphated Lead Ores. 88. Sulphated Lead Ores. 89. Tabular, White Lead Ore, from Leadhills. 90. Examination of the native Reguline Antimony, from Andreasberg. 91. Antimoniated Silver, from Andreasberg. 92. Fibrous red Antimonial Ore. 93. White Ore of Antimony. 94. *Arseniated* Olive Copper Ore. 95. *Muriated* Copper Ore. 96. *Phosphated* Copper Ore. 97. *Kryolite*. 98. Beryl. 99. Emerald. 100. Examination of Klingstone (*Echodolite*). 101. Basalt (*Figurate Trapp*). 102. Pitch Stone. 103. Addition to the Analysis of Pamice Stone (Essay 33). 104. Examination of the Jargon (*Zircon*) from Norway. 105. Examination of Madrepore. 106. Pharmacolite. 107. Scorza. 108. Examination of the Fibrous Sulphate of Barytes. 109. Tabular Spar (*Safel-spath*). 110. Examination of Micmite. 111. Examination of the *prismatic*, *Magnesian Spar*, from the Territory of Gotha. 112. Examination of the striated grey Ore of Manganese. 113. Earthy, black Oxyd of Manganese. 114. Examination of the Asphaltum from Albania. 115. Earthy brown Coal. 116. The Hungarian Pearl Stone.

* * W. N. acquaints G. S. that the supposition of Du Hamel, that what was called mild volatile alkali contains chalk, arose from a want of knowledge of the carbonic acid at the early period alluded to in his letter. The additional weight in the carbonate of ammonia arises from carbonic acid, and there is no reason to suspect any volatilization of the earth during its preparation. If any were present, it might be detected by sulphuric acid, or, in preference, by oxalic acid, either of which would carry down the lime.

*Sketch of the Orbits of the new Planets,
by Jerome de la Lande.*

Fig. 1.



Associated Copper.

Fig. 2.

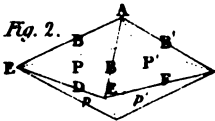


Fig. 3.

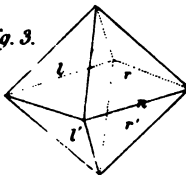


Fig. 4.

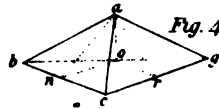
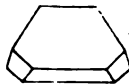
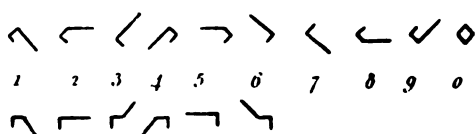
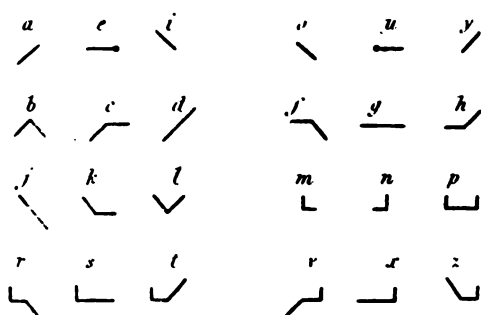


Fig. 5.





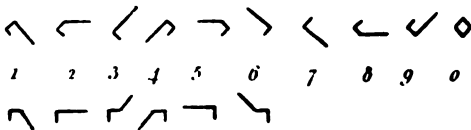
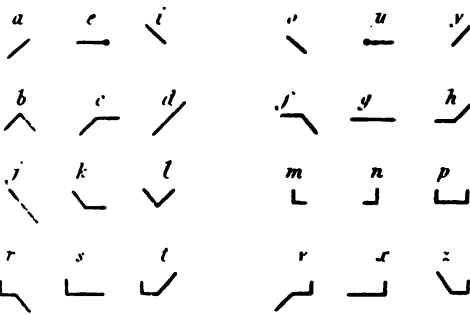
Telegraph by the human figure.



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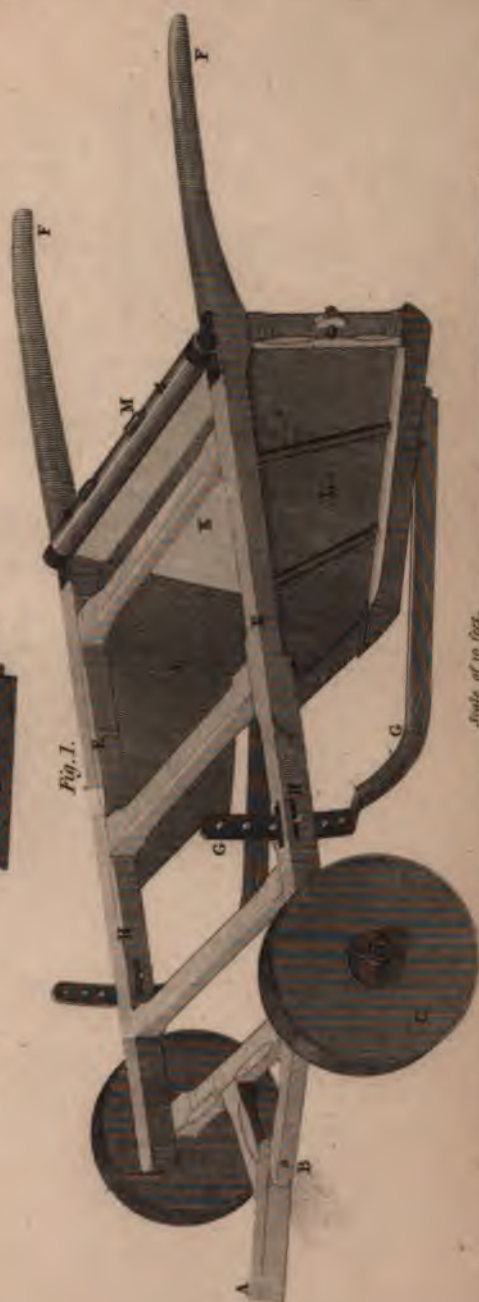
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Machine for Laying Land Levels.
by Mr. David Charles.



Scale of 10 feet.

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST, 1804.

ARTICLE I.

On the Pastes, coloured Glasses, or Enamels, of the Ancients.

*By M. KLAPROTH.**

SECTION FIRST.

THE invention of glass, which, in various respects is so highly valuable a production of art, is among those ancient discoveries of which history has transmitted us some account. Pliny relates it as follows: † “*Fama est, adpulsa nave mercatorum nitri, cum sparsi per littus epulas pararent, nec esset cortinis attollendis lapidum occasio, glebas nitri e nave subdidisse. Quibus accensus, permixta arena litoris, translucentes novi liquoris fluxisse rivos; et hanc fuisse originem vitri.*” If we suppose this account to be merely an unsupported tradition, still it contains in itself no circumstance that might render it questionable. It deserves rather the more credit, as it is hardly possible to imagine this invention could have had any other origin than that of accident.

* Read in the Royal Academy of Sciences at Berlin, October 4, 1798.—Translated by N. N. who received a copy from the author.

† Lib. XXXVI. Cap. 65.

Glass-houses at
Diospolis in the
most remote
ages,

and among the
Phenicians.

The nitrum or
natrum of the
ancients was
soda ;

or rather it con-
sisted of any salt
left upon the
ground (not
muriatic) ;

This impure salt
was not detri-
mental to the
glass.

The art of co-
louring glass is
of nearly the
same antiquity as
glass itself.

Though Pauw and some other antiquaries are more inclined to ascribe this discovery to the Egyptians, who are said to have constructed the first glass-house, in the remotest ages, at Diospolis, the ancient capital of Thebais ; yet it likewise appears, from the writings of the ancients, that this art must have arrived at a considerable degree of perfection, chiefly among the Phenicians ; as also, in general, this nation seems, in her flourishing age, to have been almost in the exclusive possession of manufactures. Sidon, that colony of theirs so flourishing by commerce, arts, and manufactures, was not less famous on account of her glass-houses. These, according to the testimony of Pliny, obtained for some hundred years the chief ingredients of their glass from the sea-shore near the Phenician town Acco, afterwards called Ptolemais, and now St. John d'Acre, in the vicinity of the small river Belus, which there empties itself into the Mediterranean.

The substance which the ancients employed for the purpose of vitrifying the sand, is comprehended by the early authors under the name of *nitrum* ; but it has long been generally agreed, that they did not mean by it our nitrate of potash, but the mineral alkali or soda ; consequently their *nitraria* were not nitre-works, but, strictly speaking, refineries of soda. And from the descriptions which Pliny and others have given of their *natrum* and its properties, it is rendered probable, that in those times all saline substances, whether efflorescing upon the soil or left by dried lakes, if not belonging to the muriatic genus, were considered as *natrum*. Hence undoubtedly, among those salts often occurred real nitre as well as native sulphate of soda.

However, such confusion in their use in manufacturing glass has not produced any real detriment ; since the longer time during which the ancients exposed their materials for glass to the action of the fire, has been more than sufficient to decompose those neutral salts, and to expel from them their acid constituent parts.

The art of colouring glass seems to be of nearly the same antiquity as the invention of making it ; as is evident not only from several passages in the ancient writers, but may also be proved by actual documents, and, among others, by the variously coloured glass-corals, with which several of the preserved Egyptian mummies are decorated. This art supposes the possession of some chemical knowledge of the metallic oxides, because these are the

only

only substances capable of producing such an effect. But it would be a problem of difficult solution to determine, what were the means and processes employed by the ancients for this purpose, as they had no acquaintance with the mineral acids, which, at present, are usually employed in the preparation of metallic oxides. It is nevertheless certain, that the art of giving many various colours to glass must, at least in later times among the Greeks and Romans, have reached an eminent degree of perfection; for they knew how to imitate, by their pastes of glass, even those gems which have a deep colour, so as to deceive the eye very considerably. A proof of this, among others, is afforded by the following words of Pliny *, relating to the artificial imitation of the *carbuncle*, a gem then in the highest estimation: “*Adulterantur vitro similitime: sed cote deprehenduntur, sicut aliæ gemmæ falsitiæ.*”

Though the ancients were ignorant of our mineral acids, they made coloured pastes in high perfection.

It was in the time of Augustus that the Roman architects began to make use of coloured glass in their mosaic decorations, besides the several species of marble and other coloured stones, which, before, were usually employed with that design. Such an application of the glass-pastes was resorted to in a villa built by the emperor Tiberius on the island of Capri, as is shewn by specimens lately found among its ruins. I subjected some of these in my possession to chemical analysis, chiefly for the purpose of discovering what metallic substances the ancients employed to tinge those variously coloured masses of glass.

Roman mosaic work of glass.

I. *Antique Red Glass.*

The colour of this glass-paste is a lively copper-red. The mass is perfectly opaque, and very bright at the place of recent fracture. This is probably the very same glass, of which Pliny says: * “*Fit et totum rubens vitrum, atque non translucens, Hæmatinon adpellatum.*”

Antique red glass; opaque.

(a) Two hundred grains of this red glass were finely triturated, and, together with 400 grains of *caustic potash*, ignited for half an hour; by which management the mixture soon entered into a thin fusion. After cooling, the whole mass was softened with water, then supersaturated with *muratic acid*; and, after this mixture had been again inspissated to a saline

Analysis, 200 grains, triturated and fused with 400 gr. potash;

softened with water; supersaturated with muratic acid; inspissated; diffused in much mass, hot water.

* Lib. XXXVII. Cap. 26.

Silex fell down. mass, it was again diffused in a large quantity of boiling water, to which a slight portion of muriatic acid had previously been added. *Siliceous earth* separated; which, collected, edulcorated and ignited, weighed 142 grains.

The remaining solution deposited crystals of muriate of lead by evaporation.

(b) The filtered solution possessed a green colour; and when concentrated by evaporation, it deposited needle-shaped crystals. When on continuing the evaporation no more such crystals would appear, the remaining fluid was diluted with spirit of wine, and thrown upon the filter. The collected crystals were washed with spirit of wine and dried in a warm temperature, upon which they weighed $32\frac{1}{2}$ grains. They consisted of muriated lead, equivalent to 28 grains of gently ignited oxide of lead.

The liquid was then supersaturated with ammonia, and alumine was separated:

The rem. fluid being sat. with mur. acid, then evap. and precip. by oxalate of potash, gave ox. of lime.

Copper was then precip. by iron.

(c) I then supersaturated with caustic ammonia the solution thus freed from its contents of lead. It was now of a dark-blue colour, and let fall a grey precipitate; which being separated, the solution was again neutralized with muriatic acid, reduced by evaporation, and upon this combined with oxalate of potash as long as any turbidness ensued. The precipitate thence formed was oxalate of lime, which after strong ignition yielded three grains of pure calcareous earth.

(d) The ingredient copper was now precipitated from the solution, by immersing into it a polished piece of iron. The reguline copper obtained by this process amounted to twelve grains, for which fifteen grains of oxidized copper must be put in the account.

Purification of the precipitated alumine.

(e) The above grey precipitate (c) thrown down by the caustic ammoniac, was mixed and digested with liquid caustic soda. When to the filtered solution, again supersaturated with muriatic acid, carbonate of soda was added, aluminous earth fell down, which after washing and ignition amounted to five grains.

Insoluble part was iron.

(f) The remaining part that was left undissolved by the caustic lye, appeared of a black-brown tinge. This, after washing and exposure to red heat, weighed two grains, and was oxidized iron.

Hence, according to this analysis, the sum of the constituent parts of the two hundred grains of the red antique glass-paste decomposed, consists of,

Silex

<i>Silex</i>	-	-	(a)	-	148 grains.	Composition of the red glass.
<i>Oxide of Lead</i>	-	-	(b)	-	28 —	
— of <i>Copper</i>	-	-	(d)	-	15 —	
— of <i>Iron</i>	-	-	(f)	-	2 —	
<i>Alumine</i>	-	-	(e)	-	5 —	
<i>Lime</i>	-	-	(c)	-	3 —	
					195	

On comparing the external characters of this red glass-paste with the cupreous scoriæ of a lively brown-red, such as is sometimes obtained on melting copper ores, it is rendered highly probable, that the ancients did not compound the above paste directly from its simple constituent parts, but instead of them have perhaps employed copper scoriæ. On that supposition they had nothing more to do, than to select the best coloured pieces to fuse and cast them into plates.

It was probably
a scoria from
copper works.

II. *Antique Green Glass-Paste.*

The colour of this green paste is a light verdigris. Its mass, like that of the preceding, is opake, and of a scoriaceous splendent fracture.

Antique green
glass; opake.
Analysis as in
§. 2.

For its chemical analysis I employed two hundred grains, which, having been treated in exactly the same manner as the foregoing, I found to consist of the following ingredients:

<i>Silex</i>	-	-	-	130 grains.	Component parts of the green enamel:
<i>Oxide of Copper</i>	-	-	-	20 —	
— <i>Lead</i>	-	-	-	15 —	
— <i>Iron</i>	-	-	-	7 —	
<i>Lime</i>	-	-	-	13 —	
<i>Alumine</i>	-	-	-	11 —	
					196 grains.

This green glass-paste, then, contains the same constituent parts as the red, only in different proportions. Both receive their colour from copper: But the reason why this metal produces in the one a red, and in the other a green colour, depends on the different degrees of its oxidation or saturation with oxygen.

Differ only in
proportion of
parts from the
green.

It is one of the chemical properties of copper, that in the state of a sub-oxide, that is only half saturated with oxygen,

it produces a copper-red enamel; while, on the contrary, when perfectly oxidized or fully saturated with that acidifying principle, the enamel which it yields is green. Pliny mentions several preparations of copper that were in use in his time; he only dwells too long on enumerating their pretended medicinal virtues. Of such artificial preparations of copper some might have been serviceable in making green glass-pastes, in the case that, perhaps, the native oxides of copper, of which in particular the copper-mines on the island of Cyprus could afford copious quantities, were not then employed for this purpose.

III. *Antique Blue Glass.*

Whether the ancients coloured glass by cobalt.

My leading object in chemically decomposing this glass, was the solution of the question: What was the colouring matter which the ancients employed in order to tinge their glass blue? The striking similarity of the colour of the blue antique glass to that of our modern, which, as is well known, is tinged by means of cobalt, has induced several learned men to conjecture, that even the ancients must have been acquainted with this fossil, as well as with its properties of colouring glass blue. This was, likewise, the opinion of Ferber, when in his *Letters from Italy*, page 114, he says: "*In the villa Adriani near Tivoli, near Fiescati, and in several places, antique mosaic works have been found which exhibited some cubes of a blue vitreous composition, and serve as a proof, that the ancients must have known the use of cobalt and the preparation of smalt.*" This opinion he repeats in various places.

The affirmative generally,

but erroneously supposed.

This opinion being supported by no chemical proof, rests solely on the supposition, that cobalt is the only substance which is capable of affording a blue enamel. However, it is certain the ancients knew the art of giving, by means of iron, a blue colour to glass resembling that which we produce by cobalt.

The contrary shown by Gmelin,

A chemical demonstration of this fact has been given by Gmelin of Göttingen, in his *Chemical Examination of a Blue Glass from an antique Mosaic Fragment**, which was found in digging a garden at Mumpelgard, and is probably of Roman

* Commentat. Götting. Vol. II.

origin. It is true that Gmelin could, in his examination, employ no more than the small quantity of a few grains; but the results were sufficient to shew, that the colouring principle in his specimen originated not from cobalt but from iron.

A like result is afforded by the following decomposition of the blue glass from the ruins at Capri. and by our author.

Its colour is a *sapphire-blue* verging towards that of smalt. Antique blue glass; almost perfectly opaque.
It is transparent on the edges only. Its fracture, as well as that of the preceding, comes nearer to the scoriaceous and conchoidal than to the splintery. Some of these blue glass-plates are particularly distinguished by this circumstance, that they are not coloured blue throughout the whole of their mass, but only to about two-thirds of their thickness. Each of the strata is so nicely distinct from the other, as to give the appearance of two plates adhering at their broad surfaces; the one blue, the other colourless. Some plates are blue only to a certain depth.

(a) Two hundred grains of the above blue paste were reduced to a subtile powder, and fused with 400 grains of *caustic soda*. Fusion with soda, &c. gave flux.
The obtained mass, softened with water, was saturated to excess, and evaporated to a moderate dryness. When redissolved in boiling water, it deposited *siliceous earth*, which, after washing and ignition, amounted to 163 grains.

(b) The fluid was then supersaturated with *caustic ammonia*. Alumina by the process with ammonia, &c.
A brown precipitate thence ensued, which, uponedulcoration, I digested with a solution of *caustic potash*. The slight portion taken up of it by this alkali was again, after saturating the lixivium with an excess of muriatic acid, precipitated by means of *carbonated soda*, and proved, uponedulcoration and red heat, to be *aluminous earth*, amounting to three grains.

(c) What remained undissolved by the caustic potash, was merely *oxidized iron*, weighing nineteen grains when ignited and washed. Oxidized iron.

(d) The liquor that had been supersaturated with caustic ammonia and possessed a blueish tinge, was by slow evaporation so far reduced to a smaller volume, that the greatest part of the muriate of soda, which had been generated and contained in it, could separate in crystals. The fluid separated from these, in which the acid predominated, and which now hardly exhibited any perceivable greenish colour, was in vain examined The liquid contained a little copper and lime; but no cobalt.

mined for cobalt *. It contained only a slight trace of copper and lime. The first of these was made to appear, by combining the fluid with *prussiate of potash*, and the brown-red precipitates thus obtained amounted to a little more than two grains, which are to be considered equivalent to about one grain of *oxidized copper*.

(e) At last, carbonated soda threw down about half a grain of *calcareous earth*.

Component parts
of the blue paste
as enamel.

In consequence of this decomposition, those *two hundred* grains of the antique blue glass-paste, must have contained the following earthy and metallic constituent parts :

<i>Silex</i>	-	-	(a)	-	163.	grains.
<i>Oxide of Iron</i>			(c)	-	19.	—
<i>Alumine</i>	-	-	(b)	-	3.	—
<i>Oxide of Copper</i>			(d)	-	1.	—
<i>Lime</i>	-	-	(e)	-	0.5	—
					186.5	

Other experi-
ments were
made but shew-
ed no cobalt.

Iron can afford
a blue enamel;
as is seen in the
smelting works.

As I have subjected the above blue glass to several other experiments, merely with an intention of discovering the cobaltic portion it might possibly contain, yet without finding the least trace of it, there appears to be no doubt, that its blue colour entirely depends on the ingredient iron. That iron, under some circumstances, is capable of producing a blue enamel, is clearly shewn by the beautifully blue coloured scoriæ of iron, which frequently are met with in the high furnaces on smelting siliceous iron-stones. But we are not sufficiently acquainted with the circumstances and conditions under which this colour is produced; for the assertion of Henckel, and some other earlier authors, that by means of iron, cemented with arsenic, the same blue tinge can be given glass which it acquires from cobalt, has not yet been sufficiently

* It is well known that nature tinges the *sapphires*, *lapis-lazuli*, *blue clays*, &c. by means of *iron* without cobalt; but man is not possessed of her means. A chemical friend, with whom the translator had a conversation on this subject and on the difficulty of proving the accuracy of the above *analysis* by a *synthetical process*, suggested the idea, that such a blue paste could, perhaps, be made without cobalt by the intermedium of *lapis-lazuli*; an idea which may afford a subject for experiment.—*Transl.*

confirmed.

confirmed. Whence, after the discovery of the blue from cobalt, the art of tingeing glass blue by means of iron has had the same fate with several other attainments now lost; namely, to have been discarded and forgotten on the account of new invented, more commodious, and certain expedients and methods.

SECOND SECTION.

THESE coloured mosaic glass-pastes of the ancients agree, The preceding with respect to their opacity and scoriaceous fracture, with our glasses resemble our enamels; On the other hand, the deceitful imitations of gems already mentioned before shew, that the ancients likewise knew how to prepare beautiful, high-coloured, and transparent glass-pastes.

But however well known those works in glass of the ancients may be, since both earlier and later writers have given sufficient information of them, and several specimens preserved in the collections of antiquaries afford a direct knowledge of this subject; it is, on the contrary, very surprising, that antiquaries are so little acquainted with that entirely peculiar and by far more remarkable painting on glass, which is formed of variously coloured delicate glass fibres, joined with the greatest nicety, and by subsequent fusion conglutinated into an homogeneous compact mass. It is formed of delicate fibres of glass united by fusion; In the earlier works on antiquities this scarce production of art is not at all mentioned; the reason of which is probably this, that the specimens now existing of it were found, perhaps, only about the middle of this (last) century.

Among later antiquaries Count Caylus appears to be the first, who in his *Collections of Antiquities* has given information, accompanied by rather inadequate drawings, of this singular species of mosaic work. Winkelman has afterwards, in his *Annotations on the History of the Art among the Ancients*, (page 5, seq.), more accurately described two other antiques of this kind, with the appellation, *Pictures made of Glass-Tubes*, in the following passage: "The works of the ancients in glass, which are not noticed in the *History of the Arts*, deserve particularly to be mentioned in this place; more especially, because the ancients carried the art of working in glass to a much higher degree than we have arrived at; a fact which, to those who have not seen their works of this kind, might

First mentioned by Count Caylus.
Ample description by Winkelman.

Very curious
antique enamel
of a duck ;

traced and fi-
nished with ex-
treme accuracy
and effect ;

and continued
through the
whole thickness of
the piece, mi-
nutely the same on
both surfaces !

It is found to
consist of threads
of glass seen
endwise.

might have the appearance of a groundless assertion." After which he mentions a floor formed of green glass-plates discovered in the Farnese-island, as well as some fragments of glass-cups, which must have been turned on the lathe, and then proceeds as follows: " But the art strongly claims our admiration in two small pieces of glass, which last year (1763) were brought to Rome. Each of them is not quite one inch long, and one-third of an inch broad. One plate exhibits, on a dark ground of variegated colours, a bird representing a duck of various very lively colours, more suitable to the Chinese arbitrary taste, than adapted to shew the true tints of nature. The outlines are well decided and sharp, the colours beautiful and pure, and have a very striking and brilliant effect; because the artist, according to the nature of the parts, has in some employed an opaque, and in others a transparent glass. The most delicate pencil of the miniature painter could not have traced more accurately and distinctly, either the circle of the pupil of the eye, or the apparently scaly feathers on the breast and wings, behind the beginning of which this piece had been broken. But the admiration of the beholder is at the highest pitch, when, by turning the glass, he sees the same bird on the reverse, without perceiving any difference in the smallest points; whence we could not but conclude, that this picture is continued through the whole thickness of the specimen; and that, if the glass were cut transversely, the same picture of the duck would be found repeated in the several slabs; a conclusion which was still farther confirmed by the transparent places of some beautiful colours upon the eye and breast that were observed. The painting has on both sides a granular appearance, and seems to have been formed, in the manner of mosaic works, of single pieces; but so accurately united, that a powerful magnifying-glass was unable to discover any junctures. This circumstance, and the continuation of the picture throughout the whole substance, rendered it extremely difficult to form any direct notion of the process or manner of performing such a work. And the conception of it might have long continued enigmatical, were it not that, on the section of the fracture mentioned, lines are observable, of the same colours which appear on the upper surface, that pervade the whole mass from one side to the other; whence it became a rational conclusion, that this kind

of

of painting must have been executed by joining variously coloured filaments of glass, and subsequently fusing of the same into one coherent body. The other specimen is of about the same size, and made in the same manner. It exhibits ornamental drawings of green, white, and yellow colours, which are traced on a blue ground, and represent volutes, beads, and flowers, resting on pyramidally converging lines. All these are very distinct and separate, but so extremely small that even a keen eye finds it difficult to pursue the subtle endings, those in particular in which the volutes terminate. Notwithstanding which, these ornaments pass uninterruptedly through the whole thickness of the piece."

Another specimen no less admirable.

Of the same glass-paste, which has been here described, The same were mention is made by Sulzer in his *Theory of the Polite Arts*, under the article *Mosaïc* (mosaïch). Having seen the piece itself in the house of its then possessor, Casanova, at Dresden, he confirms, in the capacity of an eye-witness, the description given by Winkelmann, and calls it "a remnant of antiquity, which indicates the existence of an art brought to the highest degree of perfection."

seen by Sulzer.

Mr. Townley, of London, enumerates, among the principal rarities of his celebrated cabinet of antiquities, the stone of a ring of a similar antique glass-paste, which represents a bird of fo small a delineation, that it cannot be distinctly seen but by means of a magnifying lens.

Extremely minute bird in Mr. Townley's collection.

As very few specimens of this species of glass-painting, which undoubtedly must be reckoned among the lost attainments of art, and of which even the existence is still so little known, are met with; I think it not superfluous to give the following notice of two new samples which I possess of this class of antique subjects. Both pieces have a heart-shaped form. The principal front is flat, the reverse is convex, and has from eight to ten prominences (*Ecken*). The length of one of them is one inch, the breadth four-fifths, and the thickness two-fifths of an inch. The other specimen is *two-thirds* smaller. As to colouring and manner of drawing, they are both nearly alike. The principal mass of the *larger* is of a dark-blue, and wholly opaque; but that of the *smaller* is a sapphire-blue, and in some places transparent. The blue ground is embellished with voluted, stellular, minute flowers, of so very small a delineation as to be hardly imitable by the

Two specimens in possession of the author.

Description.

pencil of the miniature painter. The colours of these flower-like ornaments, which are red, green, brown, sky-blue, and white, are pure and lively. The delineations pervade the whole substance, and upon a broken part it is seen by mere inspection, that these delicate figures have been formed of parallel glassy fibres of various colours, conglutinated by means of gentle fusion.

Coloured plate
of the larger
specimen.

As the drawings given by Count Caylus afford but a very imperfect idea of that ingenious enamel-painting, I submit here a delineation of the larger of my two specimens. *Fig. 1, Plate XIII.* represents it in its true size; *Fig. 2* exhibits it magnified.

Invention of
Meyer to multi-
ply copies of
paintings.

On this occasion the following passage from a letter of M. Kästner *, concerning an invention of the celebrated Tobias Mayer, may well deserve a place here. "Mayer possessed the art of making a number of perfectly similar copies of a painting. He compounded his picture of coloured *wax-crayons*, in the same manner as a prism may be composed of thinner prisms of the same length. Every transverse cutting afforded then a copy." Meyer might probably have been led to this method of imitation in wax, from the inspection of a mosaic work of the kind here described.

II.

On Spontaneous Inflammations. By G. C. BARTHOLDI, Professor of Physic and Chemistry.

(Concluded from Page 220.)

4. *The Fermentation of Animal and Vegetable Substances.*

Heat from fer-
mentation.

THE greater part of animal and vegetable substances, when they still retain humidity, and are accumulated into large masses, enter into fermentation, a change in their composition is effected, and they frequently heat to the point of inflammation. It is thus that magazines of hay, of turf, of flax, of hemp, stacks of hay or straw, heaps of linen-rags in paper-mills, &c. take fire spontaneously.

* *Allgemeine Geographische Ephemeriden*, by Zach, 1798.

It is principally hay which requires precaution; if the hay-harvest happen in rainy weather, it is commonly stacked before it is thoroughly dry, and in this state is more disposed to ferment and to heat. If a hay-stack is observed to be in fermentation, great care must be taken not to throw it down too suddenly, the exterior layers must be carefully detached one after the other. It almost always happens, that when a hole is made in the middle of a stack of heated hay, it takes fire suddenly.

Nothing, however, is more easy than to prevent these fatal accidents: when there is any reason to fear that the hay which is intended to be housed or stacked, is not sufficiently dry, it is only necessary to scatter a few handfuls of common salt (muriate of soda) between each layer. It would be very ill judged to regret this trifling expence; for the salt, by absorbing the humidity of the hay, not only prevents the fermentation and consequent inflammation of it, but it also adds a taste to this forage which stimulates the appetites of cattle, assists their digestion, and preserves them from many diseases.

During the great heats of summer, it frequently happens that heaps of dung inflame spontaneously: great care should be taken to sprinkle them frequently with water in the summer season, and to keep them at a certain distance from habitations, as well to prevent fires as with a view to salubrity.

5. The Accumulation of Animal and Vegetable Substances covered with an Oil.

If animal and vegetable substances heaped into a large mass, can take fire from the heat produced by their decomposition, this accident is still more to be dreaded when they are covered with oily matters, and particularly with a drying oil. Heat from greasy animal and vegetable matters.

Besides the accident which happened at the manufactory of Lagelbart, and of which our colleague Haussman gave an account to the Society, and the fire which took place in one of the finest manufactories at St. Marie-aux-Mines, there are many other examples of wool, stuffs, and pieces of cloth, which were not freed from grease, taking fire in the warehouses when they were folded together, and even while moving them from one place to another when they were in large quantities: this is principally to be dreaded when linseed oil, or any

ny other oil, drying in itself, or rendered so by oxide of lead, is employed in the preparation of these stuffs.

In the manufacture of cloths, only olive oil, or oil of colza, should be used to grease the wool.

It sometimes happens on boiling flowers and herbs in oil, which occurs in several pharmaceutical operations, that, after being taken out, the herbs dried in the oil inflame spontaneously: care should therefore be taken when these herbs are thrown away, not to heap them near other combustible bodies.

There have been several examples of vessels having been burnt in sea-ports, either by the spontaneous combustion of heaps of cordage coated with tar, or by a mixture of linseed oil boiled with lamp-black, and inclosed in a bag.

6. *The boiling of oily Matters.*

Fire from boiling
oily matters;

In the preparation of some varnishes, such as printers' ink, in which linseed-oil, boiled to a certain consistence, is generally made use of, it frequently happens that the oil takes fire, unless the necessary precautions are employed: the same effect takes place in melting butter, lard, or any other grease, if they are heated too much; so that, in these operations, it is always necessary to remove every other combustible substance, to have a lid at hand to cover the vessel as soon as the fire has caught, and particularly to take care not to pour water upon it, which instead of extinguishing it, would spread it more and give it greater activity.

7. *Torrefaction.*

and from roasted
vegetable sub-
stances,

There are many vegetable substances which increase their power of inflaming spontaneously by torrefaction, if they are inclosed in sacks of cloth, which leave them in contact with the surrounding air; such are saw-dust, roasted coffee, the meal of grain, and leguminous fruits, such as French beans, lentils, pease, &c.

There have been several instances of stables having taken fire from a bag of roasted bran which had been applied to the neck of a sick animal, and had inflamed spontaneously. The inhabitants of the country who, in some disorders of their beasts, are obstinate in applying this remedy, to which others

of

of more efficacy and less danger might be substituted, should at least be careful not to inclose the bran in the cloth, either too hot or too much roasted.

Brewers, after having made their barley and other grain, which they employ in making beer, germinate, dry it in a kiln, except what is intended for pale beer, and they generally roast it more or less highly, to give the beer a deeper or paler colour. If, therefore, when the grain is brought from the kiln, it is put, still hot, into sacks, it frequently happens that it heats and takes fire, and this has occasioned several fires in breweries.

8. *Sulphurated and phosphorated Hydrogen Gas.*

The cause of subterraneous fires and volcanoes is generally attributed to the decomposition of pyrites, or metallic sulphurets, buried in the interior of the earth. These pyritous masses are decomposed by the contact and concurrence of water and air, and the decomposition is always accompanied by a great expansion of caloric, and a disengagement of a very inflammable gas, called sulphurated hydrogen gas. This gas inflames at an elevated temperature, and can communicate the inflammation to the sulphur of the pyrites, to the coal and other bituminous matters which generally accompany it.

Similar inflammations are sometimes observed near coal-pits. In exploring the coal, veins and insulated masses of pyrites are frequently met with : since these pyrites always communicate a bad quality to the coal, the miners generally lay them aside, and throw them out of the pit : if these heaps of pyrites, mixed with coal, are then exposed to the alternate action of the sun and rain, they heat and inflame. Great care must therefore be taken that these accumulations of pyrites should be kept at a distance from all combustible bodies to which they would necessarily communicate the inflammation.

There are many operations of nature in which sulphurated hydrogen gas is formed, but it often enters into other combinations as it forms, it dissolves in water, or disengages at a temperature too low for it to inflame.

By boiling phosphorus in a solution of potash or lime, phosphorated hydrogen gas is disengaged, which, being much more combustible than sulphurated hydrogen gas, inflames at a low temperature as soon as it comes in contact with atmospheric air.

This

This gas, which in chemical experiments offers the beautiful spectacle of a fountain of fire over water, is produced naturally by the putrefaction of animal substances which are buried. The lights which are frequently seen to come out of the earth, and which are known by the name of Jack o' Lanterns, are only owing to the disengagement of this phosphorated hydrogen gas: as these lights generally appear moving about in places where they do not touch dry combustibles, they seldom occasion disagreeable accidents; but they are also disengaged in forests, and it may happen that in hot summers, when the grass and brush-wood are thoroughly dry, the gas in combustion may meet with these combustible materials, and set fire to them, and thus produce the conflagration of a whole forest: we should not therefore, too lightly, and without sufficient proof, attribute to the malevolence or to the connivance of mankind, those dreadful events which are sometimes only the result of causes purely natural.

9. *Sulphuret and Phosphuret of Lime and of Potash formed in the Combustion of several Vegetables.*

Sulphuret and phosphuret of lime or potash.

Sulphur is always formed when gypsum (sulphate of lime) or any other sulphate, whether earthy or alkaline, is strongly heated with charcoal, wood, or, generally, with any combustible which is reduced to charcoal by heat. The same salts form sulphureous waters if the remains of animal or vegetable substances are left in a water in which it is dissolved; so that it frequently requires only a little sulphate of lime, or some other sulphuric salt, to communicate the odour and taste of sulphur to water which is stagnant.

Pyrophorus.

Pyrophorus is obtained by calcining the alum of commerce, or sulphate of potash with sugar, meal, or any other substance which is reducible to charcoal.

The inflammation of pyrophorus which takes fire by the sole contact of humid air, is only owing to the sulphuret of potash, which by attracting the humidity of the air, heats to that point that it kindles the carbonaceous matter which surrounds it, and which being in a state of greater tenuity, is so much the more disposed to burn.

A pyrophoric matter may be formed in ordinary combustion.

But since many of our common combustibles contain sulphuric salts, it may happen that, in their combustion, a pyrophoric matter is sometimes formed by chance, which remains in the

residus

residue of the combustion, especially if the combustible is not entirely consumed, and a part is not reduced into charcoal, which frequently happens in the fire-places in which the combustibles are not burned in grates, and the ashes are not separated from the charcoal. There have been instances of houses having been burnt by ashes intermingled with the charcoal which had been taken too early from the fire-place and put into places where they were surrounded with combustibles, which they set on fire by a spontaneous inflammation. Happily these causes of conflagration rarely occur; for pyrophorus does not retain its property of inflaming for any length of time, and it is frequently decomposed shortly after its production, without occasioning any unpleasant event: nevertheless, care should always be taken not to put ashes newly burned, and which are still intermingled with charcoal, in places where they can communicate with combustibles.

The formation of a pyrophoric matter is principally observed in the preparation of the soda of commerce, which is obtained by the incineration of different maritime plants containing much sulphate of soda, and which, in the combustion, sometimes furnish a certain quantity of sulphur, greater or less, according to the manner in which the operation is directed.

The formation of phosphuret of lime has great analogy with that of sulphuret of lime. Although the phosphoric acid is not found so often in vegetables as the sulphuric acid, it nevertheless exists in them in greater quantity than has hitherto been believed: it is principally found in most plants which grow in marshy places, in turf, and in several species of the white woods. In reducing these woods into charcoal, a small quantity of phosphorus is sometimes formed, which may remain combined with the same bases as retained the phosphoric acid before the combustion: the phosphorus, by forming other combinations, may be rendered incapable of occasioning any accident, but it may also happen from a concurrence of several circumstances, that charcoal impregnated with any phosphuret whatever, may, by exposure to the action of a warm and humid air, disengage phosphorated hydrogen gas, which, by the contact of the atmospheric air, will take fire and communicate the inflammation to the mass of the charcoal.

Phosphoric acid is abundant in vegetables.

Two examples of this kind of spontaneous combustion have taken place in the powder magazine of Essone, in the years 8

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R

and

Spontaneous inflammation of charcoal in a powder magazine.

and 10. The first time the receiver of the machine for sifting the charcoal caught fire, and the second time it began in the magazine of charcoal, without a suspicion of any other cause except that of a spontaneous inflammation. The different reports made on these two events, have been inserted in the public journals; but the explanations that were given of them were not satisfactory. It seems very probable that they were occasioned by some phosphorus contained in the charcoal; and this explanation has the more weight, because willow (boudaine), which is used at Essone, as well as in most other powder-manufactories, and which, in many respects, deserves the preference over other woods in the preparation of powder, contains phosphoric acid, at least that does which grows in our neighbourhood.

Charcoal from turf begins to be employed in some domestic and other operations; but as it is much disposed to spontaneous inflammation, its use should be prevented, or at least it should be stored with great precaution. It has happened at Paris and other places, that magazines of this charcoal, which were unsheltered, have taken fire by the combined action of the heat and rain.

10. *Phosphorus sometimes contained in Charcoal.*

Detonation from the phosphorus in charcoal.

It may also happen that the small quantity of phosphorus which is sometimes formed in the carbonization of different sorts of wood, without uniting either with the lime or the potash, remains combined with the charcoal, which then does not disengage phosphorated hydrogen gas, nor does it readily inflame by the sole action of water or of a humid air, but which, by percussion with salt-petre (nitrate of potash) may produce a powerful detonation. It is very probable that the three successive explosions which took place in the powder-mill at the manufactory of Vonges, were partly owing to a similar cause.

Charcoal probably one cause of the bad quality of cold-short iron.

Charcoal has, in general, great influence on the different productions of nature and the arts. It is frequently observed in forges and founderies, particularly in those of iron, that the products vary according to the nature of the charcoal employed. The bad quality which is sometimes found in iron, of being brittle when cold, is generally attributed to the phosphoric acid contained in the ores: but since the same ore, by the same process,

bellies, furnishes better iron in one foundry than in another, the difference seems frequently to arise in part from the charcoal.

Such are the principal causes of spontaneous combustions, whose effects are so much the more dangerous by being least expected. The Society of Emulation thought they should render an important service to every class of proprietors, and particularly to the inhabitants of the country, by developing the physical knowledge which might guard them against dangers, of which they are too frequently the victims, from ignorance and a fatal want of foresight. I trust I have fulfilled the wish of the Society, and of the first Magistrate, whose intelligence and constant solicitude extend; without exception, to every object which may contribute to the prosperity of the country and the happiness of the governed.

III.

On the Solution of Water in the Atmosphere; and on the Nature of atmospherical Air. By Mr. JOHN GOUGH. From the Author.

To Mr. NICHOLSON.

SIR,

I DO not recollect any philosopher or meteorologist, who has attempted to demonstrate the chemical union of atmospherical air and water, by help of the following facts and arguments. Should the present endeavour, to establish the proposition, appear deserving of a place in your Journal; the insertion of it will oblige,

Yours, &c.

JOHN GOUGH.

Middleshaw, July 16, 1804.

Exp. 1. If a cylinder of dry porous wood be put into a strong glass tube, nearly of the same diameter with itself, and water be poured into the vessel, the particles of the fluid will penetrate the wood, and cause it to swell, so as to burst the glass. Some writers affirm, that the same artifice has been used with success to split rocks, an operation which is commonly performed by the elastic power of gun-powder. A quantity of

motion is generated in this experiment, which cannot be referred to the action of gravity. We must therefore ascribe it to another force, namely, the mutual attraction of wood and water in a liquid form.

Aqueous vapour attracted by dry vegetables, &c.

Exp. 2. If a piece of whip-cord or tharm be stretched by the heaviest weight, it can support, the pendent body will ascend, as oft as the string or gut contracts, in consequence of an accession of water derived from the atmosphere; on the contrary, it will descend, when the cord begins to relax from the loss of moisture. The motion generated in this instance, proves atmospherical vapour to be powerfully attracted by the dry fibres of vegetables and animals; consequently these substances have a strong affinity to water, not only in a liquid form, but also when it is diffused through the air. This affinity or force will be called the hygrometrical attraction in the sequel of the essay, for the sake of perspicuity.

The force of affinity permanent.

The preceding experiments have not the least claim to novelty; but they are the preliminaries of an inference, which is of moment in the present question. For affinity is a fixed relation of bodies, creating a disposition to coalesce, in such as are thus mutually related, as often as water is combined with another substance. The union must therefore be permanent, unless it happens to be dissolved by an external cause. Now as any certain force only gives way to another superior and contrary to itself, it is evident that a moist body, which discharges a portion of the water it contains, is obliged to part with it by a more powerful attraction, existing in its neighbourhood. It is to be remarked, that temperature is one of the external causes alluded to above; but it is disregarded at present, because the effects of its changes may be obviated in the following experiment, which is intended to throw additional light upon the hygrometrical attraction.

Hygrometrical attraction is diminished by the accession of water.

Exp. 3. Take two bibulous substances, such as two slices of sponge, or a piece of sponge and shred of woollen cloth. Make the one wet and keep the other dry; then put them both into a close vessel of glass or metal, placing them either in contact or apart: the wet body will grow lighter in a short time, and the dry one will gain more weight; this process may be prolonged, until the two substances find the equilibrium of their attractive powers; which will be accomplished, when their respective weights become stationary. This equilibrium proves the

the hygrometrical attraction to diminish with the absorption of water, and to increase with the loss of the same; consequently an union produced by this force, may be dissolved by the presence of a body, which contains less water, and therefore attracts it more powerfully.

Atmospherical air may be concluded to possess the power described above, from the changes and effects, which are observable in the following instances: First, Atmospheric air takes the water of crystallization from various salts; it therefore overcomes the affinity, which unites the component parts of these crystals. Secondly, The same hygrometer denotes a greater degree of humidity at one time, than at another, though the height of the thermometer be the same; consequently the hygrometrical attraction of the atmosphere is variable under equal degrees of temperature; because this force is evidently constant in an instrument kept in an uniform heat. Third, If two vessels be exposed, at the same time to the air, one of which contains dry potash, and the other a dilute solution of the same; the former will acquire weight, while the other grows lighter. The last fact shows, that atmospherical air may be saturated with moisture, in respect of one body, and be at the same time in a very different situation relative to another; so that evaporation evidently arises from an excess of hygrometrical attraction in the atmosphere; on the contrary, the production of dew depends upon a similar excess in the bodies on which it is formed.

I may be asked, after making this open declaration of my sentiments, which of the constituent gases of the atmosphere combine with water? The proper reply to the question appears to be this: It is atmospherical air; which I consider to be a homogeneous gas, for the following reasons:—First, The atmosphere is diaphanous; which could hardly be the case, were it a mass of uncombined fluids of different specific gravities; for, had such an arrangement been formed, the rays of the sun would have suffered a multiplicity of refractions, in their approach to the earth; and total darkness, or at best a dim twilight, would have been the consequence, had our planet been shrouded by a covering of heterogeneous gases. Thus the atmosphere appears to be homogeneous, from the consideration of its transparency.—Secondly, A given measure of oxygen is heavier than an equal bulk of azote, under similar circumstances; because smart reports are not double to sense.

Atmospherical air a simple gas. circumstances; consequently the density of the former exceeds that of the latter, supposing their elastic forces to be equal.

On this account sounds will move, in all cases, with less celerity in oxygen than they do in azote. If then our atmosphere consisted of two independent masses of these fluids, mutually pervading each other, every momentary report would have been double to sense, at a sufficient distance from the seat of sound; because such a report would arrive at the ear more expeditiously through the medium of the azote, than it would through that of the oxygen. But sounds of the shortest duration are not repeated at the greatest distances; consequently the air is homogeneous, because it is the vehicle of sound.

If the preceding arguments be just, the homogeneity of atmospherical air cannot be controverted; because the conclusions which result from the contrary hypothesis are repugnant to common experience. We come in the next place to the specific nature of this gas; but this is a difficult enquiry in the present unsettled state of chemistry, when the phenomena of galvanism are daily bringing new truths to light, and threaten to subvert the prevailing theory. Conjectures, however, will naturally spring up in the midst of uncertainty; and as a diversity of sentiment has its use in times of scientific anarchy, I will venture to propose the following hypothetical questions relative to the constitution of common air. Is not this fluid a chemical compound, having the gas called azote for its basis; to which the positive energy of the galvanic pile is united, together with water, but in a manner which distinguishes this compound from the gaseous oxide of azote? May not a gas, thus constituted, oxidate other substances through the interposition of the water, which it holds in solution by the hygrometrical attraction? Though the aqueous part of the atmosphere cannot of itself decompose common air; may not it perform the office of an intermediate agent, when assisted by the body to be oxidated, and in this manner deprive the azote of the galvanic energy, more or less perfectly, according to circumstances? Will not the aqueous vapour unite with the matter separated from the air, and produce oxygen gas, which will enter into composition with the third substance, and complete the business of oxidation? The hints suggested in the preceding queries, would have been by no means admissible in a time of more perfect uniformity in the sentiments of philosophers;

Iosophers; and nothing can be pleaded in its excuse but the revolution of opinion, which is apparently ready to take place in the theory of gaseous fluids.

IV.

*Reply to the Observations of M. l'Abbé Haüy, on arseniated-Copper. By M. LE COMTE DE BOURNON, Member of the Royal and Linnean Societies of London.**

IT is but a few days, Sir, since I had the honour to receive from you the observations which you have made on the different species of arseniated copper, described by me, in a memoir on that subject, read to the Royal Society of London, on the 19th of February, 1801. I have read these observations with the greatest interest, but not being able to adopt the opinion respecting them, to which the investigations you have submitted them have led you, I feel great obligation for the opportunity you have afforded me of explaining myself more particularly than I have hitherto done, on what relates to this interesting subject. Besides, you offer these observations with that diffidence which usually characterizes real merit, accompanied with a doubt which calls for a new examination.

Introductory remarks.

You oppose my opinion, Sir, on the division which I have made of the arseniated copper into four species, with a delicacy and a politeness, which renders the slight mineralogical discussion that becomes the necessary result of it, of infinite value to me. It is very desirable that those facts on which differences of opinion may prevail should always be discussed in this manner; the sciences would certainly gain by it, and those who cultivate them would lose nothing by yielding a little to each other.

Like you, Sir, when I employed myself on the substance which, since the first essays of the celebrated Klaproth on it, has been called a combination of the arsenical acid and copper, I thought it right to consider, under the same point of view, the different crystalline forms which it offered, deriving them all from one common base, and my first enquiries were

Notice of the former enquiries,

* Translated from the original, communicated by the author. For the paper of the Abbé, see p. 187 of our present vol.

directed

directed to determining this base, or the primitive generating crystal of all those of this substance. I was not long in discovering that among the crystals which I had subjected to this examination, there existed two forms which could not, in any way, be connected with the others: analysis has since shown, that one of these belonged to an arseniated iron, which had been improperly cited as belonging to copper, and the other to a combination of copper and iron with the arsenical acid, which had not been known before. With respect to the other crystals, as the appearance offered by each of them contradicted the opinion which connected them, it became necessary to depend in the best possible manner on all the other exterior characters which this substance could offer to the mineralogist, to attain to some result respecting it. This is precisely what I have done, and when the aggregate of these characters forced me to recognize four very distinct species in the mass of substances, which I suspected might belong to the combination of the arsenical acid and copper, I confess I experienced some satisfaction in observing that the analysis of a chemist, so justly esteemed as Mr. Chenevix is, sanctioned, in some measure, the division to which observation had led me. You remark, Sir, that these analysis, on being repeated by M. Vauquelin, varied in their result: it follows necessarily, that this support fails, or at least becomes uncertain for me: I abandon it therefore, and leave to chemistry the discussion of a fact which belongs to it, and was to me only a powerful auxiliary, to confine myself within the strict limits of mineralogy properly so called.

Preliminary observations on the mode of conducting mineralogical enquiries. But allow me, Sir, first to make some previous observations on the method which, it appears to me, should be followed to determine the union or separation of substances, and afterwards, on the possibility of finding several species placed under the combination of the same acid with the same base, but without doubt, having essential differences in the manner of combination.

The methods to be employed by the mineralogist in the study of mineral substances, are comprized in the examination of the peculiar marks which nature has impressed on each of the individuals which decorate and enrich its bosom, and which his great habit of observing has taught him to recognize. Of these marks, which we designate by the expression of exterior specific

specific characters, some are too delicate to be described; but custom enables the naturalist to seize them; their action on his sight is sudden; the most rapid glance embraces the whole of them, and the naturalist has frequently formed his opinion long before he has thought of accounting to himself for it. He is not, however, secured by them from the errors which other bulkier and more comparable characters may afterwards rectify; but the first impression received from these first traces, very often serves him as a guide in the method of employing the second. Among these latter characters, some are of easy application and almost always possible, others require attention and particular circumstances to be capable of being employed. Those which are in most common use, and easiest, are the form, the fracture, the hardness, the specific gravity, and the colour. Perhaps in a skilful hand, directed by the habit acquired from their use, these characters are almost always sufficient for the knowledge and classification of mineral substances. In stones, the colour is the most variable of all: nevertheless, it is certain, though the true cause cannot yet be assigned, that each of those which have been examined hitherto, affects one only of the known colours more readily than it does any of the others. But in the metals this character becomes more constant and more essential, and it very seldom varies without the cause of its variation being a change in the nature of the metallic substance itself.

This fact granted, when the naturalist employs the exterior specific characters, to ascertain the subject which determines his enquiry, from the moment at which the agreement of these characters, or their differences with those shown by known substances, puts him in a situation to pronounce on the identity or the difference of their nature, do not you believe that he has then the liberty of retrenching, on the one hand, those which do not agree with the opinion which he had previously thought it right to embrace; and, in the second place, to subject the others to suppositions which may occasion a change in their aspect to connect them with that which he wishes, when nature itself has not offered traces, free from doubt, of the probability of the modification which he admits in these characters?

Permit me to observe to you, Sir, that this is precisely what appears to me to be the substance of your observations on the arseniated

The consideration of some of the characters may be omitted when they do not lead to general inferences.

This has been done by M. Haüy.

arseniated copper. You seem to consider as nothing the very sensible differences which exist in the divers species which I have established, with respect to hardness, specific gravity, and colour; and, stopping at the single character of form, you make suppositions for each of them, which, in fact, terminate by connecting all those which they offer with one primitive crystal: but nature does not exhibit any of the decrements which you suppose. I have never discovered the slightest trace of them in any of the immense quantity of crystals of arseniated copper which have passed through my hands. Do you believe that these suppositions are only susceptible of being admitted in a case in which, all the other characters being agreed in the most perfect state of these substances, which is that of regular crystallization and transparency, they would become necessary only to add an accumulation of proofs to those already acquired of their identity.

Exact distinction of the species necessary to the progress of the science.

Never was more attention paid than at this moment to the great truth, that the progress of the sciences which lead to the study of nature, depends principally on the exact distinction of each of the species whose union forms the aggregate to which the science is applied. No one is more convinced of this important truth than I am. But this exact knowledge of the species, which perhaps your calculation or the analysis of improved chemistry may one day attain in a simple and accurate manner, rests at present on the agreement of the exterior specific characters. Whenever this agreement exists, we are compelled to conclude that there is a similitude in the species, and, on the contrary, a dissimilarity when they differ essentially from each other. I, however, agree perfectly with you, that before separating one of these substances from the other to make a species of each, it is requisite to be previously convinced that the differences which they offer, and on which their division rests, are not purely accidental. It appears to me, therefore, that nothing can be more undeserving of the reproach of having neglected these precautions, than, on the contrary, the establishment of the division on the invariable constancy in the difference of their exterior characters.

Chemical analysis does not supersede the mineralogical characters.

The only reason, which, in the substances in question, can raise any doubt on their difference, is the result obtained from them by chemical analysis, which constantly found the arsenical acid combined with the copper in each of them: but if the analysis

analysis had not been possible, certainly no naturalist would have hesitated to separate them from each other, according to the exterior characters shown by each of them.

Why therefore, because these substances all belong to the combination of the same acid with the same metal, should there not be found several species among them? This, I believe, is a fact which occurs much more frequently than has been hitherto supposed. Do not all the metals show various instances of striking differences in the oxides, in consequence of that which exists in the combination of oxygen with them?

The octahedral attractive oxidized iron, that which is rhomboidal, that not attractive, are not all these so many species? In a memoir which was inserted in the 75th number of the *Journal des Mines*, I have endeavoured to show that the octahedral sulphurated iron, and that in cubes, formed two very distinct species, and I do not believe that these are the only ones which exist in it. How many species are offered by sulphurated copper! I am myself acquainted with six, all perfectly distinct and characterized, which I have long intended to give the description of, but my occupations and want of time have not yet permitted me. Finally, have not you yourself been compelled to form a particular species of carbonated lime, of the arragonite, from the difference alone, which exists in its exterior specific characters, although chemistry can only find carbonic acid and lime in it?

Variations in the species of the same chemical combination.

One reason which may be alledged against the division of arseniated copper into species, is that the combination of copper with the arsenical acid being already a species in the genus of ores of copper, it would be making species of a species: and this objection, which at the first blush appears well founded, would bear equally against the various oxides, sulphurets, &c. But this difficulty seems to me to be more specious than solid: it takes its rise from the impossibility in which we still are of ascertaining every thing connected with the different causes which may produce a variation of the species. Without doubt, in this instance, for example, it is not the simple combination of the arsenical acid with the copper which forms the species, but the particular combination of this acid with the metal. Thus it is not the simple combination of oxygen, hydrogen, carbon, azote, &c. which constitutes the particular species of animal, but the manner of the combination itself.

The

Mineralogy is not yet sufficiently perfect to admit of the separation of the species by the form of the primitive molecule alone.

The mineralogical species are very accurately determined by the agreement or difference which exists in the first molecule of the formation of the different substances, but until we have fixed data to appreciate, in a determinate and invariable manner, all which relates to these molecular, the constancy or the difference in these exterior specific characters will always be the only means within our power of uniting or separating the species. I acknowledge, however, that in this case, it is necessary that this division should be established as much as possible on striking and essential characters; and I agree, at the same time, that latterly, this method has perhaps been much abused by giving importance to simple and casual characters; this has frequently placed substances in the number of species, which should only have been considered as simple varieties of those already known.

I shall now beg of you, Sir, to compare with me the different species of arseniated copper which I have described.

Comparison of the First and Second Species.

Comparison of the first and second species of arseniated copper.

The form of the first species is an obtuse rectangular octahedron, whose faces are unequally inclined. Two of them meet at the summit under an angle of 139° and at the base under one of 50° . The two others meet at the summit in an angle of 115° , and at the base in one of 65° . This octahedron is usually cuneiform: I have never perceived any modification of it.

The form of the second species is a hexahedral plate, always very thin, whose vertical planes are inclined alternately in opposite directions, so that two of them, on the same side, make an angle of 135° with the terminal faces to which they incline, and the third, one of 115° .

The most usual colour of the first species is a deep and very brilliant sky-blue, which sometimes changes to green.

That of the second species is a fine emerald green: I have never seen any other.

The specific gravity of the first species is 2881: that of the second 2548.

The hardness of the first is such that it readily cuts carbonated lime: the second is no harder than is sufficient to cut gypsum.

In your observations, Sir, you have raised to $57^{\circ} 4'$ and $65^{\circ} 8'$ the measures which I had established at 50° and 65° ; measures which you have fixed from the relations established by you between the height of one of the pyramids and the perpendiculars drawn from its bottom on the edges of its base, which corresponds with the adjacent and unequally inclined, pyramidal faces. These measures are so near to mine, that I have proved them again, and the instrument is so little capable of marking this difference, that I do not make any difficulty in adopting them.

To connect the form of the second species with the obtuse octahedron of the first, you afterwards suppose two sections made parallel to one of the most inclined faces of the octahedron, so as to detach a very thin segment, in which the centre of this octahedron is to be contained; that is to say, this octahedron is to be considerably increased on all its faces, with the exception of one alone, taken on each pyramid, and in an opposite direction in each of them. You suppose at the same time a decrement of a single row along the edges of the base, but which acts only on two of the faces of the octahedron, and that the segment which results on two of the three sides inclined to each of the terminal faces, makes with them an angle of $130\frac{1}{2}^{\circ}$ and the third 115° , measures which only differ $5^{\circ} 30'$ in the angle of $130^{\circ} 30'$ from those which I have given for the crystal.

The following is the answer dictated by the new examination which I have made of this substance.

Agreeably to what I have said in my memoir on the arseni-
ated coppers, the obtuse octahedron frequently shows slight streaks in its faces parallel to its edges, which indicates a lamellated texture in the direction of these faces. The fracture also indicates the same texture, but its fractures are always more or less irregular. I have never been able to obtain a clear one. In the second species, on the contrary, the laminae are as easily raised from the hexahedral terminal faces, as could have been done on a prism of mica. These terminal faces are sometimes streaked parallel to the edges of the sides which are inclined to them, and these streaks, which are continued strongly on the sides, never appear upon them except in this direction. This texture, very analogous to that of mica, seems to me to be totally different from that of the obtuse octahedron of the first species.

Objections to
their separation
refuted.

I have

I have submitted some new crystals of this species to measurement, and have found them agree perfectly in the measure of 115° and 135° with those which I had examined before. The angle supposed by you of $130^\circ 30'$, which I have tried on a number of crystals always appeared to me to be much too

A new variety of the second species.

small. These crystals have offered me a new variety, in which the sides of the hexahedral laminæ are less inclined on the terminal faces, with which they form an angle of about 105° . The crystal which afforded me this new variety is four lines in diameter: it is only in perfect preservation in one of its halves, but it admits of a judgment from it, that all its sides must have the same inclination. These new faces are perfectly smooth, and do not show any striæ. On another crystal, instead of these inclined sides, two planes are observed, one of which belongs to that which made an angle of 105° with the terminal face to which it inclines, and the other belongs either to that of 115° or that of 135° .

There was not any thing that I could discover connected with any of the planes of the obtuse octahedron of the first species.

Comparison of the Third Species with the First.

Comparison of the third species with the first.

The colour of the first is either a deep sky-blue or a grass-green. That most usual in the third is a yellowish green, more or less deep, but very frequently it can only be perceived by placing the crystal between the eye and the light, the intensity of the colour making the crystals appear black in every other position.

The specific gravity of the first is 2881; that of the third 4280.

The hardness of the first is not more than sufficient to scratch carbonated lime; that of the third is such as to cut fluated lime.

The first species has an obtuse rectangular octahedron for a unique and primitive crystal, whose dimensions have been given above: the figure of the third is an acute rectangular octahedron, in which each pyramid has two faces more inclined than the other two; the two most inclined faces meet at the summit, in an angle of 84° , and at the base in one of 96° , and the two others meet at the summit in an angle of 68° , and at the base in one of 102° . This octahedron is most usually cuneiform, and its prolongation is sometimes very considerable;

derable; it then takes the appearance of a rhomboidal tetrahedral prism of 84° and 96° , terminated at its extremities by a dihedral summit with isosceles triangular planes, the summit of which is placed on the edges of 84° , and the bases meet with each other in an angle of 112° . This form has not hitherto shown any other modification except being replaced by a plane, larger or smaller, on the edges of 96° . Its planes are usually very smooth and brilliant, and I have never been able to discover an appearance of division (*clivage*) in any of them.

This third species passes by the greatly lengthened octahedron to the determinate capillary variety, as well as to that which is indeterminate, and in this case the colour appears either to tend more to green or to take a more defined yellow, which sometimes has the brilliancy of gold.

The first species does not exhibit any thing which resembles these various transitions; it is always the same obtuse octahedron, and only varies by a very slight prolongation of its crystals parallel to the least inclined faces. To make the formation of the acute octahedron of the third species, secondary to the obtuse one of the first, you suppose a decrement at the base of the latter of two rows above and below the edges of the union of the least inclined faces, and another of four rows at that of the union of the most inclined faces, and by this, you get an acute octahedron, whose most inclined faces meet at the summit, in an angle of $86^\circ 24'$ and at the base in an angle of $93^\circ 36'$; and the others meet at the summit in an angle of 71° , and at the base in one of 109° .

I acknowledge that this approximation to the measures which I have given is seducing, and, considering the natural smallness of the crystals of this species, it would perhaps be difficult for me to pronounce determinately whether the measures which I have taken are much more exact than those to which you have attained by calculation; but this I can assure you, that no indication whatever, in either of these two octahedra, leads to the supposition which has given you this result.

From the details I have now given, it is easy to deduce the reasons which impel me to adhere to the division which I have thought it right to make in the arseniated copper, and prevent me from adopting the approximation to which your ingenious hypotheses have led you. Every thing still seems to me to tend

Reasons for adhering to this division of the species.

to indicate a difference in the species into which I have separated them, while to bring them to a single one, you have been obliged to consider as nothing, all the exterior specific characters, with the exception of the form alone, and you have used the latter only in establishing an hypothesis respecting it, to which, neither artificial means, such as splitting, nor natural indications, such as secondary planes on a primitive crystal, nor the retaining of primitive planes on the others have led you. If in the explanation of embarrassing facts, it were permitted thus to make nature speak when she is silent, I have no hesitation in asserting that a naturalist so well informed, and so practised in the art of calculation as you are, would find very few obstacles in resolving all the species into each other whenever he chose.

Of the four species of arseniated copper which I have described, there still remains one, in respect of which you have not made any calculation of approximation; it is the fourth, which, as I have stated, has for a primitive crystal a tetrahedral prism with an equilateral triangle for a base. Nevertheless you do not exclude it when you draw your conclusions on the doubt which you believe to exist on the division of the arseniated coppers into four species, and you direct this doubt equally to the fourth. Since according to your supposition this can only be as a primitive to the first crystal, that it may also be in a state to be brought to it, I have thought it right to add likewise the comparison of this fourth species with the first.

Comparison of the Fourth Species with the First.

Comparison of
the fourth
species with the
first.

The colour of the first species is a deep sky-blue, which sometimes changes to grass-green. That of the fourth is a brilliant deep verdigris, but its surface is very readily discoloured, doubtless by oxidating, and it then becomes black: this renders the crystals opaque, which, when they have not experienced this alteration, are beautifully transparent; this is very unusual among those which have been naturally exposed to the free air during a certain time. This change however exists only at the surface; by scratching the crystals lightly, their fine colour is readily restored to them. I have never perceived any thing resembling this fact, which unquestionably depends on the nature of the substance of this species, either among the crystals of the first, or among those of the second and third.

The

The specific gravity of the fourth species is 4280, and is consequently perfectly analogous to that of the arsenated copper of the third species, but at the same time greatly inferior to that of the first, which is 2881.

Its hardness, much below that of the third species which we have just seen that it resembles in weight, is also less than that of the first species, by which it is cut.

Its forms, which are greatly multiplied, while there exists but one in the first species, differ essentially from those of the first.

All these forms appear to me to be derived from the right tetrahedral prism with equilateral triangles for bases; and all those which I have endeavoured to recognize and have given in my memoir, seemed to me to be very readily derived from that, as I have also said: these crystals are always extremely small, and I was unable to measure them; a few groups of them exhibited this prism in such a manner that I could observe it perfectly complete. The scarcest crystals, after those belonging to the varieties which I have represented at figures* 15, 16 and 17, and which are usually so grouped as to penetrate each other, and thus to become very difficult to be known, are the very acute complete rhomboid and its incomplete varieties, such as are represented at figures 22, 23 and 24 of my memoir. I even hesitated, the division (*clivage*) not having shown any thing to guide me, whether I should not take this rhomboid for the primitive form. In this instance, the only manner which seemed to me natural and simple of connecting this crystal with the obtuse octahedron of the third species, is to suppose this octahedron become rhomboidal by an increase having taken place by the superposition of laminæ, or by the collection of rows of molecularæ, growing smaller, on only one of the faces of each pyramid, and in an opposite direction on each of them, as occurs in the spinel, and a number of substances having a right octahedron for a primitive crystal, and which I have also seen in the diamond. But in this case, either the increase must be made on the most inclined faces, and then calculation shows that the planes of the rhomboid should have $57^{\circ} 39'$ and $122^{\circ} 21'$ for the measure of the angles of its plane; or the same increase must be made on the least inclined faces, and then the measures of the angles of the plane of the rhomboid

Reasons for
forming a fifth
species.

* Phil. Trans.

Reasons for
forming a fifth
species.

should be $47^{\circ} 4'$ and $132^{\circ} 56'$: now although from its minuteness, the rhomboid which exists in this species is not measurable by instruments, yet I can affirm with certainty that it is much more acute than either of the two which this supposition gives rise to.

It is very true, Sir, that since my memoir on the arseniated copper was printed, I have thought it right to separate the species described in it, and to make a fifth of one of the substances included among them; a substance which is entirely different from the others in its exterior characters, and seems to lead naturally to a belief that water must be a principal in the number of its component parts: but you are in an error with respect to which of these substances I believe to be really of a different nature from the others. In my first work on the arseniated copper, I found it necessary to make several subdivisions or varieties in the fourth species: of these, the three first are the determinate capillary, the indeterminate capillary, and that which is solid at one of its extremities, and divided into very delicate fibres at the other. You seem to believe that I comprehend these three varieties in the new species which I have been led to consider as an hidro-arseniate. This opinion would indeed be, as you very justly observe, completely contradictory of every thing which I have said on these varieties, in the description I have given of them, and I have no doubt, must have astonished you. The only species of arseniated copper to which this opinion is directed, is that which includes the two varieties to which I have given the names of hematiform and amiantiform: they are very certainly the same, with this difference, that one is the product of the decomposition of the other.

This arseniated copper, when it is unmixcd, forms very compact mamellæ, but striated from the centre to the circumference, and very often also forming different concentric layers: their colour is brown, sometimes with a very slight tinge of green. This substance, in its aspect, greatly resembles the hematiform oxide of tin, which, in *Cornwall*, bears the name of *wood-tin*; this has caused the miners of the same country to give the name of *wood-copper* to this species of arseniated copper. Its hardness, notwithstanding its fibrous texture, is sufficiently great to scratch fluated lime with facility. Its specific gravity is from 4100 to 4200.

This

This substance very readily changes; it then passes to an ash-grey, and loses considerably of its hardness; it also frequently experiences a more advanced decomposition, and then becomes perfectly white, and so soft that the nail is sufficient to cut it and separate its fibres. If the mamellæ which have passed to this state are broken, their decomposition is frequently found to have reached their centre; but very frequently also the centre has perfectly retained its brown colour and its hardness, and both are observed to diminish gradually on approaching the circumference; if in this case the attention is directed to the fibres near the circumference, it will be seen that they are detached from each other, and that the surface itself of these mamellæ has the appearance of that of madrepore, from the immense number of fissures in different directions, which are occasioned by the contraction. At length this substance arrives at such a degree of decomposition, that the mamellæ open completely, their fibres are entirely separated from each other, and, in this state, frequently become so slender and flexible as perfectly to resemble small portions of papyraceous amianthus.

Reasons for
forming a fifth
spec.es.

Such, Sir, is the nature of the arseniated copper, in which I believe I have found properties and a mode of existence which differ from those of the others, and which I have had opportunities of examining with greater facility and accuracy since the impression of my memoir. This easy decomposition, the prodigious contraction, and the great change which this substance experiences in itself, have led me to suppose that the loss of water has great influence on it. But this fact is, however, nothing more than an opinion, which experience will either reject or confirm; and I have so stated it. It may also be very possible that this arseniate is no more than a variety of the third species, as I at first considered it. You must, however, acknowledge that it offers very singular characters, of which it would, therefore, be interesting to know the cause.

The dealers in minerals from London, and principally Mr. Mawe, have, I believe, taken a collection of arseniated copper to Paris. This substance appears to me worthy of engaging the attention of chemists, who, I am of opinion, should repeat the analyses of it. Perhaps they may one day throw a clearer light on a substance which has greatly interested me, and to which I am at this moment indebted for the pleasure of having entered with you, Sir, into a discussion, agreeable in its manner and instructive in its effect.

V.

Process for separating Alumine from Alum. In answer to the Enquiries of R. T. By Mr. FREDERICK ACCUM.

To Mr. NICHOLSON.

SIR,

IF you have not received a better answer to the queries of your correspondent R. T. (p. 141) who finds so much difficulty in obtaining alumine in a state of purity, &c. I request you will give a place to the following lines in your next. I have endeavoured to be as concise as possible, and have the honour to be,

Sir,

Your most obedient humble servant,

FREDERICK ACCUM.

Old Compton-Street, Soho,

July 17, 1804.

Common process for separating alumine from alum, throws down a salt, and not a mere earth.

The process in general recommended by systematic writers for obtaining alumine, (which R. T. adhered to) is absolutely erroneous, for no pure earth of alum can be expected if a solution of alum of commerce be decomposed by a solution of carbonated alkali, and subsequent ablution and expulsion of the carbonic acid. For alum of commerce is a triple compound, consisting of alumine, potash and sulphuric acid in excess. If we attempt to saturate this excess of acid, by the addition of an alkali, or even by pure alumine, a highly insoluble salt, (sulphate of alumine) is generated, differing from alum principally in the proportion of its constituent parts. When we therefore gradually add (as R. T. did) to a solution of alum, a carbonated alkali, the first effect of it is, to saturate the excess of the sulphuric acid, and the precipitate consists principally of the salt, which is insoluble in water. A farther addition effects instantly a decomposition of part of the salt, which, in proportion as it takes place, becomes mixed with the alumine, which is thus covered or defended from the further action of the alkali. This being the case, it is obvious that no subsequent washing can do more than separate the sulphate of potash, and therefore the residuum, instead of being pure alumine (as R. T. imagines) contains also a variable portion of true sulphate of alumine.

To

To prove what has been stated before, let any quantity of the obtained alumine be heated in contact with charcoal powder; introduce the mixture into a tabulated retort, connected with the pneumatic apparatus, and add to it muriatic or sulphuric acid; the result will be sulphuretted hydrogen gas in abundance, particularly if heat be applied to the mixture. The production of this gas will become evident on considering the philosophy of the process. By means of the following method, alumine may be obtained in a purer state.

Take any quantity of alum of commerce, dissolve it in four parts of boiling distilled water, and mingle this solution with liquid ammonia till no further cloudiness ensues. Then heat the mixture nearly to the boiling point for a few minutes, and transfer it on a filter. In proportion as the fluid passes off, pour more water over the precipitate, and continue the ablu-
 tion till the water runs off tasteless. Having done this, let the precipitate while yet in a pasty state, be transferred into a basin or flask, and add to it muriatic acid, in small quantities at a time, until the whole is dissolved; then evaporate the solution till a drop of it, when suffered to cool on a plate of glass, yields minute crystals. If it now be suffered to cool, crystals of alum will be deposited. Remove these crystals, by decanting the fluid, and renew the evaporation, until on further cooling, no more crystals are formed. Nothing now but nearly pure alumine remains in solution; the potash and sulphuric acid being got rid of, at the expence of a little alumine in the crystals. The fluid may therefore be diluted with water, and then decomposed by liquid ammonia, taking care to add this alkali in excess. The precipitate thus obtained, when washed and dried, will be alumine in a state of considerable purity.

The alumine thus obtained does *not* yield sulphuretted hydrogen when heated with charcoal power; it emits *no* odour when breathed upon, it is somewhat unctuous to the touch, insipid, and white as snow.

The precipitate or supposed earth, if heated with charcoal, and then treated with an acid, gives sulphuretted hydrogen.

Method of separating pure alumine.

Precipitate by ammonia; and wash well. Dissolve in muriatic acid. Evaporate and crystallize for a contaminating portion of alum. Then dilute and precipitate again by ammonia.

The earth that now falls is considerably pure.

VI.

Short Account of Mr. Arthur Woolf's Improvement in the Construction of Steam-Engines.

Discovery that heated steam may be suffered to expand, with a very powerful effect;—

MR. WOOLF founds his improvements on an important discovery he has made respecting the expansibility of steam when increased in temperature beyond the boiling point, or 212° of Fahrenheit's thermometer. It has been ascertained for some time by Mr. Watt, that steam acting with the expansive force of four pounds per square inch against a safety-valve exposed to the atmosphere, is capable of expanding itself to four times the volume it then occupies, and still to be equal to the pressure of the atmosphere. Mr. Woolf has discovered and established by proof, that steam of the force of five pounds the square inch, may expand itself to five times its volume; that masses or quantities of steam of the like expansive force of six, seven, eight, nine, or ten pounds the square inch, may be suffered to expand to six, seven, eight, nine, or ten times its volume, and will still be respectively equal to the atmosphere, or capable of producing a sufficient action against the piston of a steam-engine to cause the same to rise in the old engine (with a counterpoise) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines first brought into effect by Messrs. Boulton and Watt;—that this ratio is progressive, and nearly if not quite uniform, so that steam of the expansive force of 20, 30, 40, or 50 pounds the square inch of a common safety-valve will expand itself to 20, 30, 40, or 50 times its volume with like effect; and that, generally, as to all the intermediate or higher degrees of elastic force, the number of times which steam of any temperature and force can expand itself is nearly the same as the number of pounds it is able to sustain on a square inch exposed to the common atmospheric pressure: provided always that the space, or vessel in which it is allowed to expand itself, be of the same temperature as that of the steam before it was allowed room to expand.

—the increase of temperature not being considerable.

Respecting the different degrees of temperature required to bring steam to, and maintain it at, different expansive forces above the weight of the atmosphere, Mr. Woolf has

, by actual experiment, setting out from the boiling of water, or 212° , at which degree steam of water is equal to the pressure of the atmosphere, that in order to give it an increased elastic force equal to five pounds the pressure, the temperature must be raised to about $227\frac{1}{2}^{\circ}$, it will have acquired a power to expand itself to five times its volume, still being equal to the atmosphere, and capable of being applied as such in the working of steam-engines, according to his invention: and with regard to various other pressures, temperatures, and expansive forces of steam, they are shown in the following table:

of the relative pressures per square inch, temperatures and expansibility of steam at degrees of heat above the boiling point of water, beginning with the temperature of steam of an elastic force equal to five pounds per square inch, and extending to a pressure able to sustain forty pounds on the square inch.

Pounds per square Inch.		Degrees of Heat.		Expansibility.	
n of affic pre- nat- over ref- of atmo- e a /- .	5	requires to be maintain- ed by a tempera- ture equal to about	227 $\frac{1}{2}$	5	times its volume, and con- tinues equal in elasticity to the pressure of the atmo- sphere.
	6		230 $\frac{1}{2}$	6	
	7		232 $\frac{1}{2}$	7	
	8		235 $\frac{1}{2}$	8	
	9		237 $\frac{1}{2}$	9	
	10		239 $\frac{1}{2}$	10	
	15		250 $\frac{1}{2}$	15	
	20		259 $\frac{1}{2}$	20	
	25		267	25	
	30		273	30	
	35		278	35	
	40		282	40	

Table of pressures, temperatures and expansions of steam equal in force to the atmosphere.

and so in like manner, by small additions of temperature, expansive power may be given to steam to enable it to expand to 50, 60, 70, 80, 90, 100, 200, 300, or more times its volume, without any limitation but what is imposed by the fragile nature of every material of which boilers and their parts of steam-engines have been or can be made; prudence dictates that the expansive force should never be carried to the utmost the materials can bear, but rather be considerably within that limit.

Upon this discovery, Mr. Woolf has obtained a patent for his improvements in the steam-engine, from the specification

Extract from
Mr. Woolf's
specification.

tion of which, the following extract is made, which will be sufficiently instructive to those who are acquainted with the subject.

"If the engine be constructed originally with the intention of adopting my said improvement, it ought to have two steam vessels of different dimensions, according to the temperature or the expansive force determined to be communicated to the steam made use of in working the engine; for the smaller steam vessel or cylinder must be a measure for the larger. For example, if steam of forty pounds the square inch is fixed on, then the smaller steam vessel should be at least one fortieth part the contents of the larger one; each steam vessel should be furnished with a piston, and the smaller cylinder should have a communication both at its top and bottom (top and bottom being here employed merely as relative terms, for the cylinders may be worked in a horizontal or any other required position, as well as vertical): the small cylinder, I say, should have a communication both at its top and bottom with the boiler which supplies the steam, which communications, by means of cocks or valves of any construction adapted to the use, are to be alternately opened and shut during the working of the engine. The top of the small cylinder should have a communication with the bottom of the larger cylinder, and the bottom of the smaller one with the top of the larger, with proper means to open and shut these alternately by cocks, valves, or any other well-known contrivance. And both the top and bottom of the larger cylinder or steam vessel, should, while the engine is at work, communicate alternately with a condensing vessel, into which a jet of water is admitted to hasten the condensation, or the condensing vessel may be cooled by any other means calculated to produce that effect. Things being thus arranged, when the engine is at work, steam of high temperature is admitted from the boiler to act by its elastic force on one side of the smaller piston, while the steam which had last moved it has a communication with the larger steam vessel or cylinder, where it follows the larger piston now moving towards that end of its cylinder which is open to the condensing vessel. Let both pistons end their stroke at one time, and let us now suppose them both at the top of their respective cylinders, ready to descend; then the steam of forty pounds the square inch entering above the smaller piston will carry it down-

wards,

wards, while the steam below it, instead of being allowed to escape into the atmosphere or applied to any other purpose, will pass into the larger cylinder above its piston, which will take its downward stroke at the same time that the piston of the smaller cylinder is doing the same thing; and while this goes on, the steam which last filled the larger cylinder, in the upward stroke of the engine, will be passing into the condenser to be condensed during the downward stroke. When the pistons in the smaller and larger cylinder have thus been made to descend to the bottom of their respective cylinders, then the steam from this boiler is to be shut off from the top and admitted to the bottom of the smaller cylinder, and the communication between the bottom of the smaller and the top of the larger cylinder is also to be cut off, and the communication to be opened between the top of the smaller and the bottom of the larger cylinder; the steam, which in the downward stroke of the engine filled the larger cylinder, being now open to the condenser, and the communication between the bottom of the larger cylinder and the condenser shut off; and so on alternately, admitting the steam to the different sides of the smaller piston, while the steam last admitted into the smaller cylinder passes alternately to the different sides of the larger piston in the larger cylinder, the top and bottom of which are made to communicate alternately with the condenser.

"In an engine working with the improvements which have been just described, while the steam is admitted to one side of the piston in the smaller cylinder, the steam on the other side has room made for its admission into the larger cylinder, on one side of its piston, by the condensation going on on the other side of the large piston which is open to the condenser; and that waste of steam which takes place in engines worked only by the expansive force of steam, from steam passing the piston, is prevented; for all steam that passes the piston in the smaller cylinder is received into the larger.

"In such an engine, where it may be more convenient for any particular purpose, the arrangement may be altered, and the top of the smaller made to communicate with the top of the larger, and the bottom of the smaller with the bottom of the larger cylinder; in which case the only difference will be, that when the piston in the smaller cylinder descends, that
in

Extract from
Mr. Woolf's
specification.

in the larger will ascend, and while the latter descends the former will ascend, which for some particular purposes may be more convenient than the arrangement before described."

Mr. Woolf then proceeds to describe various other modifications of his invention, and points out means for applying his improvements to the working of steam-engines already constructed and now in use. It is obvious that the advantages of this discovery promise to be very considerable, and I shall take the earliest opportunity of communicating the facts as they shall be brought forward in practice.

VII.

*Description of a Chemical Lamp, with double concentric Wicks,
Communicated by Mr. FREDERIC ACCUM.*

Description, &c.
of an improved
chemical lamp.

THOSE who are familiar with Chemistry will readily allow, that one of the principal obstacles which frequently impede the progress of the young chemist in the prosecution of his science, is the want of proper apparatus: he is at a loss to select from the number of instruments displayed in the lectures of his teacher, those which are calculated for rendering, *in his own apartment*, the most important truths of the science legitimate by experiment.

It must naturally be so, as long as chemists are anxious to exhibit a variety of costly and complicated apparatus, and continue to pay that frivolous regard to *show* which characterises so many public lectures; and as long as the student is told in the introductory lesson, that the science cannot be learned but in the laboratory, fitted up with furnaces, stills, bellows, water-baths, sand-baths, &c. It is true indeed, that many chemical phenomena cannot be accurately observed without the help of instruments calculated to assist the imperfection of our senses; but it is equally true, that many of the brilliant apparatus which are daily displayed in the laboratories of teaching chemists, as instruments of research, serve more to divert the attention of the auditors, than to elucidate the fundamental truths of the science.

The modern processes of philosophical enquiry differ so much from what they formerly were, and the instruments of experiment

ment have been so much improved during our own time, that by means of a comparatively small number of them, the most complicated processes may very commodiously and conveniently be carried on in the closet of every cultivator of chemistry.

*Description, &c.
of an improved
chemical lamp.*

To awaken the industry of the junior class of chemists, I gave in the twenty-fourth number of this Journal, a description and sketch of a convenient and portable universal furnace, and in the twenty-third number of the same Journal, I ventured to recommend to the young analyst a cheap and useful apparatus for drying the products of his analysis, and also convenient for experiments on artificial cold, &c. The favourable reception which these trifles have met with amongst those amateurs of the science for whom I have caused them to be constructed, has encouraged me to point out to them an improved lamp-furnace, by means of which, in the small way and on the table of the student, almost every operation may be performed at a cheap rate, as well as with facility and dispatch. In order to enable the intelligent reader to judge for himself, I shall first detail the construction of the instrument, and then point out to the inexperienced operator some of the most capital processes which serve to unfold the fundamental truths of the science, and for which the lamp may be applied according to the conditions laid down.

Fig. 1, Plate XIV. is a perspective view of the improved chemical lamp-furnace. It consists of a brass rod screwed to a foot of the same metal, loaded with lead. On this rod (which may be unscrewed in the middle, for rendering it more portable) slide three brass sockets with straight arms, terminating in brass rings of different diameters. The largest measures four inches and a half. These rings serve for supporting glass alembics, retorts, Florence-flasks, evaporating-balons, gas-bottles, &c. for performing distillations, digestions, solutions, evaporations, saline fusions, concentrations, analyses with the pneumatic apparatus, &c. If the vessels are not wished to be exposed to the naked fire, a copper sand-bath may be interposed, which is to be previously placed in the ring. Each of the brass-rings may, by means of a thumb-screw acting on the rod of the lamp, be set at different heights, or turned aside, according to the pleasure of the operator. Below these rings is a fountain-lamp on Argand's plan, having a metallic valve within,

Description, &c.
of an improved
chemical lamp.

within, to prevent the oil from running out while the reservoir is put into its place. This lamp also slides on the main brass rod by means of a socket and thumb-screw. It is therefore easy to bring it nearer, or to move it farther, at pleasure, from the vessel which may remain fixed; a circumstance which, independent of the elevation and the depression of the wicks of the lamp, affords the advantage of heating the vessels by degrees after they are duly placed, as well as of augmenting or diminishing the heat instantly; or for maintaining it for several hours at a certain degree, without in the least disturbing the apparatus suspended over it. It may therefore be used for producing the very gentle heat necessary for the rectification of ethers, or the strong heat requisite for distilling mercury.

The chief improvement of this lamp consists in its power of affording an intense heat by the addition of a second cylinder added to that of the common lamp of Argand. This additional cylinder incloses a wick of one inch and a half in diameter, and it is by this ingenious contrivance, which was first suggested to me by Mr. Webster, that a double flame is caused, and more than three times the heat of an Argand's lamp of the largest size is produced. This part of the construction of the lamp is clearly shewn in *Fig. 2*, which represents the concentric wick-holders of the lamp; the distance between the exterior and interior cotton is half an inch, the circumference of the largest wick is $4\frac{1}{2}$ inches, and that of the smaller two. Both the wick-frames are connected by a fine screw cut upon a piece of pinion-wire.

Fig. 3. is a section of the concentric cylindrical tubes in which the wicks move.

The superior advantages of this lamp, above all others I am acquainted with, consists therefore in quickly producing, if required, a very low as well as intense heat, and in regulating its power instantaneously; by means of which the operator may observe a number of minute circumstances essential to be known, but which cannot be noticed when the same process is carried on within a furnace.

Use of the Lamp-Furnace.

From what has been stated it is obvious, that this lamp may be used for a variety of chemical operations, if conducted under the conditions here pointed out; a few of which are—

1. For

1. For obtaining oxygen gas, by means of the glass retort, Description, &c. of an improved chemical lamp. from a mixture of four parts of finely powdered black oxide of manganese, and three of concentrated sulphuric acid; or by merely heating in a retort, hyper-oxygenized muriate of potash.

2. For disengaging nitrogen, or azote, from animal substances, by affusing two parts of nitric acid of commerce, diluted with an equal bulk of water, upon one part of muscular flesh (a piece of beef or veal cut into small pieces), and heating the mixture to ebullition in a glass retort.

3. For obtaining hydrogen, sulphurated and phosphorated hydrogen, in the usual manner; or for producing heavy carbonated hydrogen by strongly heating, in a retort, a mixture of three parts, by weight, of concentrated sulphuric acid, and one of sulphuric ether, or two of alcohol; nitrous gas, by any of the usual processes; gaseous oxide of nitrogen, by decomposing nitrate of ammonia by heat in a retort; sulphureous acid gas, by causing four parts of concentrated sulphuric acid to act on one of lump-sugar, assisting the action of the mixture in the retort by heat; muriatic acid gas; oxygenized muriatic acid gas, and carbonic acid gas, according to any of the usual methods; ammoniacal gas, by heating in a retort a mixture of two parts of finely powdered lime and one of muriate of ammonia.

4. For the distillation of nitric, muriatic, oxi-muriatic, acetic, oxalic, arsenic, prussic, suberic, mucous, and camphoric acids, according to the methods recommended by systematic writers.

5. For the production of metallic, earthy, and alkaline sulphurets; such as sulphuret of potash, soda, barytes, strontia, ammonia, iron, copper, tin, mercury, &c.

6. For performing the analysis of ores of gold, silver, copper, lead, zinc, tin, &c. and for examining mineral and native salts, earth and stones, according to the methods pointed out in the "Practical Essay on the Analysis of Minerals," and for a variety of other operations, too numerous to be detailed.

Old Compton-Street, Soho.

July 18, 1804.

VIII.

*On the mutual Precipitations of Metallic Oxides. By
J. L. GAY-LUSSAC.**

The subject is
hitherto but
little known.

Utility of the
investigation.

THAT we have still so little information on the mutual precipitations of metallic oxides, can only be attributed to the complication of the results which they offer. Indeed, a little reflection will convince us that the oxidation, the affinity, the reciprocal action of the oxides, and the property which they have of neutralizing the acids unequally, are so many causes which must join in the production of the phenomena. It would, nevertheless, be very useful to know the order in which the metallic oxides precipitate from their solutions; the chemical analyses, and more especially the purification of metallic salts, would thence become easier. It was with this intention that I have made some experiments, and if they have not been sufficiently numerous to have enabled me to distinguish the influence of every cause, they, at least, show that of some, and will serve to draw the attention to a subject which is still obscure and very complicated.

I shall begin by relating the results which I have obtained, and shall afterwards endeavour to determine their causes.

Experiments
with the green
and red muriates
of iron.

Having taken a solution of green muriate of iron, I added to it a little red muriate of the same metal, and I poured potash into the mixture in a quantity at least sufficient to decompose all the red muriate separately. After agitation, the first portions of alkali yielded a precipitate of iron very much oxidized, without any mixture of black oxide; but by adding more and more of the alkali, it finished by being composed of the two oxides. The filtered liquor was then perfectly limpid, and no longer produced a blue with the prussiates, nor a black with the gallic acid: this proves that the very oxidized iron had been precipitated from it. On making the inverse experiment, that is to say, on putting a little green muriate of iron into a larger quantity of red muriate, and precipitating it by the alkali, the black oxide was retained in the solution to the last, and was not precipitated until after all the red oxide. Hence therefore it results that the black oxide

* From the *Annales de Chimie*, No. 145. or Vol. XLIX. p. 21.

of iron precipitates the red oxide, and that it is, consequently, ^{*}very easy to have green solutions of iron without red oxide. Black oxide of iron precipitates the red.

Into a solution of sulphate of zinc of commerce, which is known to contain much iron, I poured a little potash, to produce a precipitate, and I agitated and heated the mixture. Experiments with the sulphate of zinc of commerce. On examining the precipitate I found oxide of zinc and a little iron very much oxidized, and nevertheless the liquor still contained much iron, but it was at the minimum of oxidation: an addition of alkali only separated oxide of zinc. I divided the filtered liquor into two portions; into one I poured oxygenated muriatic acid, and I boiled the other with a little nitric acid. Potash then poured into the two liquors separated all the iron from them, so that there only remained a very pure sulphate of zinc, containing only a little sulphate of potash, which it is easy to avoid by employing oxide of zinc, recently prepared and well washed, to separate the iron. On making the same experiments on other solutions of zinc, I constantly found that oxide of zinc precipitated red oxide of iron, and that, on the contrary, it was precipitated by the black oxide. Oxide of zinc precipitates red oxide of iron, but is precipitated by the black.

A solution of zinc in nitric acid may be directly obtained, sufficiently pure, by dissolving it very rapidly: great part of the very oxidized oxide of iron is precipitated; and that which remains in solution requiring a great excess of acid, is precipitated by dissolving another quantity of zinc. But if the solution has been made slowly, it retains much iron, which being but little oxidized, is retained very strongly.

It is well known that, in all experiments of this kind, the precipitate may be composed of one or of two oxides, according to the quantity of alkali employed; but the better to observe what passes, it is advisable to put very little alkali into the metallic solution, the precipitate being in that case composed of only one oxide. The quantity of alkali employed should be small.

By continuing to follow the same processes I found, that when the iron is very much oxidized it is precipitated by oxide of copper, and that the inverse takes place when it is but little so. Importance of the mutual precipitations of copper and iron. Here are two very important consequences, because they may be very frequently applicable in the arts: the first is, that all the iron may be separated from a solution of copper; the second, that all the copper contained in a green solution of iron may be abstracted.

Several

Sulphate of copper may be freed from iron :

Several colours are prepared with sulphate of copper, but the iron which it always contains, and which hitherto has never been completely separated, alters the shade. If, to accomplish this last object, the iron is strongly oxidized by means of nitric acid, or, which is better, of oxygenated muriatic acid, the sulphate of iron may be intirely precipitated by pouring a sufficient quantity of potash into it, and by heating and agitating the liquor.

and sulphate of iron, from copper.

Green sulphate of iron is also frequently employed in the arts, and in many of them it is desirable that it should not retain any copper. Iron has the property of separating it, but it appears that it only does so imperfectly, and requires much time. It would doubtless be more advantageous to employ potash, and to pour a little into the green sulphate : the precipitate of black oxide of iron would be speedily re-dissolved by agitation, and would precipitate, at the same time, the oxide of copper and the red oxide of iron, if there was any in the green sulphate.

Ammonia dissolves oxide of iron at a minimum.

I shall here remark that, having employed ammonia to discover the copper in the green sulphate, I observed that, on adding an excess of alkali, the oxide of iron was dissolved in great abundance, though it is known that, in the same circumstances, it does not dissolve the very oxidized iron. The solution left in the air is decomposed, the ammonia escapes, and a black crust is formed on the surface of the liquid which soon defends it from the contact of the air. In analyses, ammonia is frequently employed to separate iron, but this method is not good unless it had been strongly oxidized before. This induces me to believe that this circumstance might have prevented Bergman from separating iron from nickel by means of ammonia; for he found that his solution contained the oxides of both metals, and this could only have arisen from the iron not having been sufficiently oxidized.

Oxygenated muriate of mercury precipitates the red oxide of iron and those of zinc and copper
Oxide of silver precipitates oxide of copper.

I also discovered, by the same means, that the oxide of oxygenated muriate of mercury precipitates the red oxide of iron with the greatest facility, and those of zinc and copper from their muriatic solution.

Having dissolved a piece of silver in nitric acid, I obtained a blue liquor composed of copper and silver. A little potash poured into the solution formed a flocculent precipitate, composed,

posed, in great part, of oxide of silver, because the precipitation took place only where the alkali was poured in; but this precipitate was gradually covered with oxide of copper, and, by agitation, it was in a little time replaced by the latter. An addition of alkali having given me a precipitate of oxide of silver which was not re-dissolved, I filtered it, and obtained a liquid perfectly colourless, which did not contain any more copper. If it is wished to avoid the potash in the solution, a part of the impure nitrate of silver may be decomposed separately, and the precipitate, well washed, may be employed to separate the copper of the other part. This simple method of separating copper from a solution of silver may be very useful in laboratories, and even in large works.

The oxide of silver also decomposes the nitrate of zinc; and the oxide of manganese the muriate of copper.

In what precedes I have, for shortness, supposed that the precipitates were pure oxides; but I am far from believing this to be the case; on the contrary, I consider nearly all of them as true salts. Copper, for example, was always precipitated of a bluish-green, although the shade varied with the oxides which were precipitated; and it is now well ascertained by Proust and the younger Berthollet, that the green and blue oxides of copper retain some of the acid.

The precipitates are not pure oxides.

Such are the facts as I have observed them, and on which alone I shall make some reflections. Although they are too few to have enabled me to fix on all the circumstances which concurred in their production, the examination of them will, however, develop some of them.

In fact, if we direct our attention to the acidity of the different salts noticed above, we shall see,

Inferences from a consideration of the state of the acidity.

1st. That the iron which is but little oxidized, and the highly oxidized mercury which precipitates the red oxide of iron, the oxide of zinc, and that of copper, approach nearer to neutralization than the latter.

Neutralization of the acids by oxides.

2^d. That the zinc and manganese which precipitate the copper, neutralize the acids better than it*.

* By neutralizing the acids more or less, I mean that property possessed by the metallic oxides, and some earths, such as glucine and alumine, of approaching more or less, in their combinations with the acids, to the term of neutralization.

3d. That the oxide of silver which precipitates those of zinc and copper, neutralizes the acids better than them.

Alumina is precipitated by metallic oxides;

and by glucine.

Magnesia precipitates the earths and oxides.

If, besides, we reflect, that alumina, whose solutions are very acid, is precipitated by several metallic oxides which neutralize the acids better than it; that, according to the experiments of Vauquelin, glucine decomposes aluminous salts, and that its solutions are more neutral than those of alumina, although they are not entirely so; and, finally, that magnesia, which neutralizes the acids perfectly, precipitates the preceding earths, and a very great number, not to say all, the oxides from their solutions; we cannot abstain from allowing that, if the property possessed by the metallic oxides and several earths of neutralizing the acids unequally, is not the only cause of the decompositions which I have detailed, it is at least one of the principal.

The affinity for oxygen is not the cause.

We may also conclude from the same experiments, that the metals which have a great or a weak affinity for oxygen, do not enjoy any particular property with respect to their mutual precipitations; for we see that iron, in a state of great oxidation, is precipitated by a number of oxides which it precipitates when it is less so; and, that there are several oxides which contain less oxygen than that of zinc, which precipitate the latter, while there are others which are precipitated by it.

The affinity of the different metals for oxygen is therefore rejected as the cause of the mutual precipitations of their oxides; but can the greater or less oxidation of the same metal occasion a variation in the affinity of the oxide for the acids?

The state of the oxidation influences the affinity of the oxides for the acids;

This opinion has been promulgated by Cit. Berthollet in his *Chemical Statics* *, and he has grounded it upon several facts, in which the metal, by losing a little of its oxygen by any means whatever, forms another salt with less acid. This happens to the oxygenated muriate of mercury, which, by exposure to light, or by being brought into contact with iron, is changed into white muriate by abandoning some of its acid. Although these, and other similar facts, are capable of a different interpretation, other considerations, which I shall omit here, because they would lead me too far, induce me to participate in the opinion of Citizen Berthollet; but I do not be-

* A translation of which will be published by Mawman, in the *Poultry*, about the middle of the present month.

lieve that this cause can have much effect, it being strongly counteracted by the acidity which almost all the metallic solutions possess, and by the insolubility of the oxides. Thus, although it appears to me that iron, when little oxidized, has more affinity for muriatic acid than when it is greatly oxidized, I should rather attribute the precipitation of the latter by the former to the great excess of acid which its solution requires, than to its weaker affinity.

Neither, for the same reasons, do I believe that the affinity of the different oxides for the acids, an affinity which I measure, with Cit. Berthollet, by the capacities for saturation, can be considered as the cause of their mutual precipitations. but is not the cause of their mutual precipitations.

Besides, there is one consideration of some importance which should be taken into the explanation of the mutual precipitations of the oxides; it is that, in a case where the precipitation of a metallic solution is produced by means of an alkali, the precipitate retains some of the acid which can favour its solution; so that an oxide which could retain much of the acid, would dissolve more readily than that which could only retain less. It must really be so with iron, which, when it is precipitated from a green solution, retains much more acid than when it is precipitated from a red solution, and which dissolves much better in the acids in the first case than in the second. This more ready solution cannot, however, be considered as a cause of the mutual precipitations of the metallic oxides; it may be very favourable to them, but cannot determine them. In fact, we see that the oxide of copper, which retains much acid, is nevertheless precipitated by oxide of silver, which does not sensibly retain any, when they are precipitated from their nitric solutions by potash. Influence of the retained acid.

CONCLUSION.

THE metallic oxides are mutually precipitated from their solutions. Several causes may contribute to this; but among the number of the principal must be placed the property which they have of neutralizing the acids unequally. General inferences.

This property has furnished us with the means, 1st. Of freeing a green solution of iron from the red oxide which it may contain; 2^d. Of separating the sulphate of zinc and that of copper from the iron which is always found in them; 3^d. Of having a green sulphate of iron free from copper; 4th. Of

General inference.

readily separating the copper from a solution of silver. It is easy to accumulate these applications by extending them to a greater number of substances. Thus, the oxides of cobalt and nickel do not neutralize the acids equally; that of cobalt which neutralizes it most, will be able to precipitate the iron and remain alone in the solution. Thus, also, since iron neutralizes the acids much better than highly oxidized iron, it will be easy to separate this metal from its solutions, by oxidating it strongly, and afterwards precipitating one part of the solution, to employ it, after being well washed, to precipitate the iron of the other part.

The greater or less affinity of the metals for oxygen does not give them any particular property with respect to the mutual precipitation of their oxides.

Oxidation produces a variation in the affinity, or the capacity for saturation of the oxides for the acids; nevertheless, the results are only sensible inasmuch as they produce a change in the neutralization, and in this case they may be attributed to the latter cause.

The affinity of the oxides for the acids may indeed be attributed to their mutual precipitations, but its effects are limited.

It appears, therefore, in general, that, all circumstances remaining otherwise the same, the substances which neutralize the acids best, may precipitate the others from their solutions.

I repeat in concluding, that it is only on the facts which I have related that I have established my reasoning, and it was not my object in this note to speak of the precipitations by the metals, nor of those which are owing to the reciprocal action of the oxides, or to that of the latter and the alkalis.

IX.

*Report of the State of his Majesty's Flock of Fine Woooled Spanish Sheep, for the year ending Michaelmas, 1803. By the Right Honourable Sir JOSEPH BANKS, Bart. P. R. S. &c. &c. From the printed Copy communicated by the Author.**

THE wether lambs of the last year having been sold in their State of his Majesty's flock of Spanish sheep, and the rams wool retained, in order that two years growth might be prepared for sale together, his Majesty's Spanish flock consisted, when shorn in June 1802, of ninety-six ewes only; the fleeces of these, after having been washed on the sheeps' backs as usual, weighed as follows:

In wool, as shorn from the sheep	-	-	352 lbs.
Loss in scowering	-	-	96
Amount of scowered wool	-	-	256

This wool, when sorted, produced as follows:

Prime wool, or R.	221 lbs. at 5s. 9d.	£.63 10 9
Choice locks, or F.	32 — 3 6	- 5 12 0
Fribs, or T.	3 — 1 9	- 0 5 3
		£.69 8 0

After deducting the expence of sorting and scowering, at the high rate which an individual who is not a manufacturer must pay for these processes, this wool is worth about 5*l.* a tod, or 43*l.* 5*s.* a pack, as clipped from the sheeps' back.

The prime wool was purchased by John Maitland, Esq. ^{sale of the} member of parliament for Chippenham, whose mercantile ^{wool;} house, established for more than a century, has always dealt largely in the importation of Spanish wool, and who from the first introduction of Merino sheep into this country by the King in the year of 1787, has uniformly given the most liberal and zealous aid to the promotion of his Majesty's patriotic views, though doubtful in the beginning of the ultimate success of the project.

* For the original project of this important undertaking and its subsequent progress, see the former reports by the same Right Hon. Gentleman, inserted in our Journal, quarto, vol. IV. p. 289, and octavo, vol. V. p. 65.

manufactured
into cloth;

It was made into cloth by Mr. Edridge, a manufacturer of Chippenham, whose skill and respectability in his line is exceeded by no man. He inspected its quality with the most minute exactness, and with an eye more inclined to expect symptoms of degeneration than of improvement, during the whole of the numerous processes to which wool is subjected in the making of broad cloth, and he found that in every one of them it answered to his complete satisfaction.

The cloth made from this wool proved so excellent in its kind, that the King was graciously pleased, at the desire of Mr. Maitland and Mr. Edridge, to permit these gentlemen to explain, in his Majesty's presence, its qualities and peculiarities.

of which samples
may be seen.

Samples of this cloth may now be seen in Mr. Maitland's warehouse in Basinghall-street; and it will be found, in conversing with Mr. Maitland and his partners, that in their opinion the R.'s of his Majesty's wool, considered as a pile, are inferior to but few of the best of those imported from Spain, though it is probable that no pile in Spain throws out so small a proportion of F.s and T.s. From this opinion it may fairly be deduced, that his Majesty's wool has improved since the sheep were imported from Spain; indeed there is every reason to believe that it is still improving, and will in a very few years equal if not excel the very best piles that have hitherto been imported into this kingdom.

The wool has
improved.

Improvement of
the carcase of
Merino sheep,
by Mr. Tollet,
without dam-
aging the
wool.

Mr. Tollet, a gentleman of Gloucestershire, who has purchased Merino sheep both from the King and from Lord Somerville, has been very successful in improving the carcase without damaging the wool; he possesses a ram, bred from a ram and a ewe both purchased from the royal flock in 1801; which, when clipped in June last, yielded 11 lbs. 12 oz. of unwashed wool. The carcase of this sheep was then estimated by good judges at 16 lbs. a quarter, and it was admitted to be a handsome sheep.

For this annum Mr. Tollet has refused an offer of 200 guineas, or of 100 for the next season's use of him; he also refused 50 guineas each for the sire and the dam, though old and infirm, being unwilling to part with animals which had belonged to the royal flock; he however sold their ram lamb of the last year for 30 guineas, and thus made some progress in ascertaining the value of this important breed.

Tollet

These facts, which prove an amelioration in the King's Merino sheep, are fully confirmed by the improved shape and weight of his Majesty's sheerling rams of the present year, and give a justifiable hope, that by a due selection of rams and ewes, and a correct judgment in matching them, Merino sheep will in time be produced, with carcases perfectly fashionable, and wool as perfectly fine.

No purchaser having been last year found for the lambs wool at a price adequate to its value, it was made into light ladies cloth, which proves excellent, and promises to be a valuable article. A speculation, however, has offered for manufacturing the lambs wool into superfine woollen hose, which seems likely to yield a still better price for the raw article than the cloth.

The demand for his Majesty's Merino sheep increases at present beyond all calculation. The best informed clothiers in Gloucestershire, enlightened no doubt by the useful labours of the Bath Society, and the valuable experiment of Dr. Parry, as well as by the Doctor's, and by Lord Somerville's publications, are among the most anxious applicants to purchase. The Bath Agricultural Society, whose attention has been most particularly directed to the improvement of English wool, humbly requested the King to give them a Spanish ram, which request his Majesty most graciously complied with last Autumn, and they returned thanks in the warmest terms of respectful gratitude and satisfaction.

As speculation on the value of Spanish sheep is evidently on the increase, and a reasonable probability now appears that his Majesty's patriotic exertions in introducing the breed, will at last be duly appreciated and properly understood, it would be palpably unjust should the views of those who wish to derive a fair advantage from the sale of the progeny of Spanish sheep purchased by them from the royal flock, be in future impeded by a continuation of the sale of the King's sheep at prices below their real value.

This circumstance having been stated to the King, his Majesty was graciously pleased to permit the rams and ewes that are to be parted with from the royal Merino flock this year, to be sold by auction, in the same manner as is done at Woburn by his grace the Duke of Bedford, and at Holkham by Mr. Coke, on the presumption of this being the most likely manner of placing

Confirmation by
the improve-
ment of his Ma-
jesty's flock.

Lamb's wool.

The demand
from the growers
proves that these
facts are well
established ;

and render it
just that the
sheep should be
distributed by
the competition
of a public sale.

placing the best individuals of their improved breeds in the hands of persons most likely to preserve, and further to improve them.

JOSEPH BANKS.

August 17, 1803.

POSTSCRIPT.

Postscript.

AS the publication of this report has been delayed by unavoidable circumstances to so late a period, it is proper to add, that the wools of 1803 have yielded, both raw and scoured, much as usual. The prime or R. of the ewe flock were sold for 6s. 9d. a pound, and that of the Rams for 6s. 6d. These enormous prices, however, depended on a scarcity of imported Spanish wool, and are highly distressing to the manufacturer: they ought not, therefore, to be allowed to enter into the speculation of the grower.

Notice of sale of the sheep that can be spared.

The sheep that can be spared from the royal flock will be sold by auction this year at a barn opposite the Pagoda in Kew-lane, on the 15th of August next. Notice of the particulars will be given as soon as possible.

July 10, 1804.

X.

*On the Oxides of Lead. By THOMAS THOMSON, M. D.
Communicated by the Author.*

Great importance of the analysis of metallic oxides.

PERHAPS there is no practical branch of chemical investigation of more importance than the analysis of the metallic oxides. Almost every thing relating to the metalline paints and salts depends upon it; and it involves, either directly or indirectly, most of the interesting questions in the theoretical department of chemistry.

first applied with effect by Bergman and Scheele.

Bergman and Scheele were, I believe, the first persons who applied chemical analysis, in the modern sense of the phrase, to the metallic oxides. Notwithstanding the difficulties with which these illustrious chemists had to struggle, their experiments were made with so much care and sagacity, that they still furnish us with the best data for ascertaining the composition of several of these bodies. No modern chemist has laboured

pouré so successfully in this department as Mr. Proust. To him we are indebted for the analysis of the oxides of zinc, iron, tin, copper, antimony, and arsenic. His dissertations are all stamped with the character of originality, and display so much skill and candour, that they never fail to command the confidence of the reader. If he sometimes pushes his consequences a little too far, he more than compensates for this by the originality of his views, and the new light which he throws upon every subject that he discusses.

I intend at present to offer some observations on the oxides of lead, a subject more than once slightly touched upon by Proust, but never fully discussed by him. I trust the difficulty of the subject will plead my excuse, if I shall be unfortunate enough to fall into mistakes.

We are acquainted with three oxides of lead sufficiently distinct from each other. The first is of a yellow colour, and forms the base of almost all the salts of lead; the second is a paint well known by the name of *red lead*; the third a brown powder discovered by Scheele, and examined more lately by Proust and Vauquelin. Besides these three, a fourth has been announced by Proust; and *litharge* has been considered by some as constituting a fifth. Let us examine these oxides.

I. *Yellow Oxide.*

The yellow pigment called *massicot* consists essentially of this oxide; but the easiest method of forming it, is to dissolve lead in nitric acid. Pure lead dissolves completely in that acid; but the lead of commerce usually leaves a small quantity of grey powder, which consists for the most part of oxide of antimony, sometimes mixed with a little silica. When the solution is concentrated by evaporation, we obtain crystals of nitrate of lead, a salt too well known to require any particular description.

1. When the crystals of nitrate of lead thus obtained by evaporation, and well dried upon blotting-paper, are exposed to a temperature of about 300°, they lose, at an average, three *per cent.* of their weight. This loss is not to be ascribed to the escape of mere water, for the fumes smell strongly of nitric acid.

2. When 69 grains of lead are dissolved in nitric acid, and the solution evaporated to dryness, the nitrate of lead, after being

being dried at the temperature of 300° , weighs 112 grains. Hence 100 grains of lead yield $162\frac{1}{2}$ grains of nitrate of lead. From this we learn that 100 parts of nitrate of lead consist of

$$\begin{array}{r} 61\frac{1}{2} \text{ lead,} \\ 38\frac{1}{2} \text{ foreign bodies.} \\ \hline 100. \end{array}$$

Carbonate of
lead: white
lead.

3. When 112 grains of nitrate of lead (dried at 300°) are dissolved in water and mixed with a solution of carbonate of potash, a copious white powder precipitates, which is a carbonate of lead. Bergman shewed long ago, that the white lead of commerce is precisely the same with this carbonate. When washed, collected on a filter, and dried at 300° , it weighs 90 grains. This shews us, that 69 grains of lead yield 90 grains of carbonate: of course, 100 grains of lead would yield $130\frac{1}{2}$ grains of carbonate. From this experiment we learn, that 100 parts of precipitated carbonate of lead are composed of

$$\begin{array}{r} 76\frac{1}{2} \text{ lead,} \\ 23\frac{1}{2} \text{ foreign bodies.} \\ \hline 100. \end{array}$$

Carbonate of
lead by heat
leaves yellow
oxide; 9 lead +
1 oxygen:

4. When 90 grains of precipitated carbonate of lead are exposed in a retort to a heat gradually raised to redness, the acid and water which they contain are driven off, and a yellow coloured oxide remains behind. This oxide weighs 77 grains, and contains, of course, 69 grains of lead. Hence it follows, that the yellow oxide of lead is composed of 69 lead + 8 oxygen, or *per cent.* of

$$\begin{array}{r} 89.7 \text{ lead,} \\ 10.3 \text{ oxygen.} \\ \hline 100. \end{array}$$

— very fusible.

It is well known that the oxides of lead very easily melt and run into glass. This happens in the preceding experiment, unless particular care be taken. In that case the lead acts with great energy upon the retort; but the loss of weight is the same, unless the heat has been a great deal too high. When the oxide is fused in an earthen vessel, it covers the surface with a yellow glass, as in the coarsest kinds of pottery. In that case some of the oxide may be dissipated, unless the proper

per precautions are taken. It deserves attention, that when carbonate of lead is slowly heated in a glass retort till it begins to melt, the melted portion has a fine yellow colour, while the colour of what remains in the state of a powder is a dirty pale brick-red; whereas in a platinum crucible the melted portion is red and the unmelted yellow.

5. From the preceding experiments it follows, that the yellow oxide of lead contains 10.3 *per cent.* of oxygen. Mr. Proust has deduced nine *per cent.* as the proportion of oxygen, from his experiments. This result does not differ much from mine. If I have committed an error, the oxygen I think is rated too high; for the lead which I used contained $1\frac{1}{2}$ *per cent.* of antimony, the oxides of which have much more oxygen than the yellow oxide of lead. Perhaps we shall come nearer the truth by taking the mean of the two results: we may therefore consider the yellow oxide of lead as composed of

$$\begin{array}{r} 90\frac{1}{2} \text{ lead,} \\ 9\frac{1}{2} \text{ oxygen.} \\ \hline 100. \end{array}$$

6. The preceding experiments enable us to state the constituents of nitrate of lead as follows:

1. Nitrate dried on Blotting-Paper.	2. Nitrate dried at 300°.
66 yellow oxide,	68.5 yellow oxide,
34 acid and water.	31.5 acid and water.
100	100.0

Comp. parts nitrate of lead, &c.

They give us also the precipitated carbonate of lead, dried at 300°, as follows:

$$\begin{array}{r} 86 \text{ yellow oxide,} \\ 14 \text{ acid and water.} \\ \hline 100. \end{array}$$

The native carbonate of lead contains about 16 *per cent.* of carbonic acid. Precipitated carbonate then either contains less acid than native, or it loses a part at a low heat. It is well known that carbonates, when in crystals, frequently contain more acid than when in the state of powders.

7. Yellow oxide of lead is a powder of a lively yellow colour, tasteless, insoluble in water, but soluble in fixed alkalies and acids.

Characters and habitudes of the yellow oxide.

acids. The alkaline solutions have a yellow tinge; but the acids are most frequently colourless. It readily melts when heated, and forms a yellow, semitransparent, brittle, hard glass. It does not lose oxygen gas when heated. In violent heats a portion of the oxide is dissipated. When kept heated in the open air, its surface becomes brick-red. When mixed with metallic lead it runs, according to Proust, into a green glaze.

Yellow oxide by mere heat from the nitrate.

8. The yellow oxide may be obtained directly from the nitrate of lead, by exposing that salt to a sufficient heat; but the loss of weight sustained is usually greater than it ought to be. I suspect that this is one reason why Proust found so small a proportion of oxygen in yellow oxide. One hundred grains of nitrate of lead (obtained by evaporation) were put into a small Wedgewood crucible furnished with a lid, and enclosed in a common earthen-ware crucible. They were exposed for half an hour to an intense red heat in a wind-furnace. The salt was converted into a very hard, yellow, brittle glass, nearly opaque: It had sustained a loss of 40 *per cent.* or about six *per cent.* more than it ought to have lost. On breaking this glass to pieces the reason of this became obvious: It contained a great number of globules of lead reduced to the metallic state, some of them of considerable size. From this experiment we learn, that lead is reducible directly from the nitrate merely by the application of heat, without adding any combustible matter.

II. *Supposed first Oxide.*

Ashes of lead, or supposed first oxide. It is yellow oxide and metallic lead.

When lead is kept melted in the open air, it is soon covered with a dirty coloured powder, formerly called the *ashes* of lead. When this powder is heated sufficiently, it melts into a greenish yellow glass, in which globules of lead may be detected. Mr. Proust has shewn, that these ashes are a mixture of the yellow oxide of lead with lead in the metallic state. They do not, therefore, constitute a peculiar oxide.

Neither is the *white oxide* of the French chemists entitled to a place among the oxides of lead; being in all cases nothing more than the yellow oxide combined with an acid, usually the carbonic.

Proust's oxide by boiling lead in the nitrate.

But Mr. Proust, in his observations on the *Connoissances Chimiques* of Fourcroy, has mentioned the method of forming an oxide of lead containing less oxygen than the yellow. When
lead

lead is boiled in a solution of nitrate of lead, the liquid gradually assumes a yellow colour, and, on cooling, deposits crystals in scales. These crystals, according to Proust, contain the oxide in question: But his conclusions, as far as appears, were formed from the single experiment related. He does not seem to have decomposed the salt, nor to have examined its base.

1. When 100 grains of nitrate of lead are dissolved in water, The experiment, and boiled in a phial with a cylinder of lead (weighing $64\frac{1}{2}$ grains), the metal soon loses its brilliancy and is covered with a white crust, while the liquid assumes a yellow colour. The boiling was continued (water being added as fast as it evaporated) till the liquid seemed to exert no farther action on the lead. The cylinder being then taken out and weighed, was found to have lost 44 grains. From this we learn, that 100 grains of nitrate of lead dissolved in water, are capable of uniting with 44 grains of lead, or almost half their weight. The whole, however, was not dissolved. A bluish-grey powder fell to the bottom, and increased in quantity as the cylinder diminished. If these 44 grains were oxidized at the expence of the yellow oxide of the nitrate, we should have a new oxide containing much less oxygen; and it would be easy to assign the proportion of its constituents; for 100 grains of nitrate contain 66 grains of yellow oxide, composed of $59\frac{1}{4}$ lead and $6\frac{1}{4}$ oxygen: Therefore the new oxide contains $59\frac{1}{4} + 44$ lead and $6\frac{1}{4}$ oxygen, or, *per cent.*

$$\begin{array}{r} 94.3 \text{ lead,} \\ 5.7 \text{ oxygen.} \\ \hline 100.0 \end{array}$$

But it is extremely unlikely that the 44 grains of lead should receive the whole of the oxygen necessary to enable them to dissolve from the oxide, while an excess of nitric acid is present in the solution. Let us therefore examine the new salt.

2. When the solution cools, it deposits thin scaly crystals of Deposition of a light yellow colour: They have the same sweet astringent scaly crystals by cooling, and taste as common nitrate, but are less soluble in water. If the small needles by yellow liquid which remains be farther concentrated, it depo- evap. White sits, on cooling, small needles of a pale yellow colour, not depos. of carbo- unlike *sugar of lead*. Their taste is sweet and astringent; they nate by cold are not altered by exposure to the air. When thrown into cold water they fall to the bottom, the liquid gradually be- water. comes

comes milky, and deposits a white powder. This powder does not disappear, though the solution be heated boiling hot; but the liquid acquires the property of dissolving an additional quantity of the salt, without depositing any more white powder. Boiling water dissolves the salt without any similar deposition. Hence I think we may conclude, that the white powder is owing to the presence of some carbonic acid in the cold water, and that our salt in this respect resembles acetate of lead.

The salt deposited in the boiling, contained yellow oxide and an under proportion of acid;

3. When 30 grains of the salt deposited during the boiling of the lead in the nitrate, were cautiously heated to redness, they melted into a yellow mass, which weighed 24.5 grains. The loss of 5.5 grains must be ascribed to acid and water. Hence this salt is composed of

81.5 oxide,
18.5 acid and water.
100.0

These 24.5 grains of oxide being dissolved in nitric acid, yielded 35 grains of common nitrate of lead (dried at 300°). But 35 grains of nitrate contain 24 grains of yellow oxide, which scarcely differs from the quantity dissolved. From this experiment it seems to follow, that the salt in question contains only yellow oxide, and that it differs from common nitrate in containing a smaller proportion of acid. But it will be said, perhaps, that the oxide of the salt absorbed oxygen from the nitric acid during the application of the heat, and was thereby oxidized up to the state of yellow oxide.

as also the crystals do.

4. Twenty-three grains of the needle-form crystals were dissolved in water and decomposed by carbonate of potash. The carbonate had the common appearance, and, when dried in 300°, weighed 24 grains. But 24 grains of common carbonate contain about $21\frac{1}{2}$ of oxide, and ought therefore, when dissolved in nitric acid, to yield about $31\frac{1}{2}$ grains of common nitrate of lead (dried at 300°); and, upon trial, I found this to be the case very nearly. The oxide in the needle-form crystals then is the yellow; for there is no apparent course from which, in the above experiment, oxygen could be drawn. And if this be the case with the needles, it must be so also with the scaly crystals; for the two salts are obviously the same.

5. Sixty-

5. Sixty-three grains of the salt, partly in scales and partly in needles, were fused with carbonate of potash in a Wedgewood crucible. By solution and filtration a flesh-coloured powder was obtained, which was a mixture of oxide of lead and filica. It weighed 53 grains; but a portion which I could not estimate adhered to the crucible. The filica was obviously abraded from that vessel. Thirty grains of this powder digested in nitric acid, left $3\frac{1}{2}$ grains of filica; of course, $26\frac{1}{2}$ were dissolved. The solution yielded $39\frac{1}{2}$ grains of nitrate of lead. Now $39\frac{1}{2}$ of nitrate contain 27 grains of yellow oxide, or almost the very quantity dissolved. The oxide obtained by this experiment, then, was the yellow; of course, it coincides exactly with the preceding ores.

The oxide of Proust's salt is not different from the yellow.

Proust's salt, then, does not appear to contain a different oxide from common nitrate; but its new properties were owing to the different proportion of its acid. It is completely neutralized, whereas common nitrate contains an excess of acid, and is, in fact, a super-nitrate. But if this conclusion be well founded, Proust's nitrate may be formed by exposing common nitrate to a heat sufficient to expel the excess of acid. It was requisite to verify this presumption by experiment.

6. One hundred grains of nitrate of lead (dried in 300°) were exposed to a graduated heat in a flask. Fumes of nitrous acid separated in abundance, and the salt lost five per cent. of its weight. On increasing the temperature the salt melted into a transparent glass of a very pale yellow colour. The weight of the mass was now reduced to 85 grains. Hence it was composed of 68.5 oxide and 16.5 acid, or, per cent. of

80 oxide,
20 acid.

100.

On pouring water into the flask and digesting, I obtained a yellow solution similar to that formed by boiling lead in nitrate of lead, but not so deep. A yellow powder refused to dissolve; it consisted chiefly of the portion of salt at the bottom of the flask, which had been exposed to a higher temperature. It was tasteless, and not unlike sub-muriate of lead. When heated to redness it melted into a yellow glass, and lost 14 per cent. It was therefore composed of

86 oxide,
14 acid and water.

100.

The solution being evaporated; deposited two sets of crystals; one set consisting of common nitrate of lead, another set resembling those obtained by Proust.

Three nitrates of lead; viz. neutral, and with excess or with defect of acid.

7. From the preceding details we learn, that there are three distinct species of nitrated lead: The first is a *super-nitrate*, or contains an excess of acid; the second is neutral; the third contains an excess of base, and is, of course, a *sub-nitrate*. The first species includes the common nitrate of chemists in all its varieties; the second, the nitrate of Proust; the third, the yellow powder obtained by heating common nitrate sufficiently.

III. Brown Oxide.

Brown oxide; left when minimum is dissolved in nitric acid, &c.

Though this oxide contains a maximum of oxygen, I beg leave to introduce it here, because the knowledge of its composition is necessary to enable us to analyse the *red oxide* of lead. It was discovered by Scheele, and described by him in his dissertation on manganese. When diluted nitric acid is poured upon red lead, the greater part of the oxide is dissolved, but a brown powder remains behind, which is not acted upon by the acid. This brown powder is the brown oxide of lead. Proust discovered that it may be formed also by causing a current of oxi-muriatic acid gas to pass through red lead suspended in water.

Its habitudes;

1. This oxide is a tasteless powder, of a flea-brown colour, and very fine and light. It is not acted on by sulphuric nor nitric acid. To muriatic it gives out oxygen, and converts it into oxi-muriatic acid. Oxi-muriatic acid dissolves it, and forms two salts, muriate and hyper-oxi-muriate of lead. The vegetable acids reduce it to the state of yellow oxide. Fourcroy, on the authority of Vauquelin, affirms, that sulphur takes fire when triturated with brown oxide of lead. With me the experiment did not succeed: I suspect, therefore, that the oxide used by Vauquelin contained a portion of hyper-oxi-muriate of lead mixed with it.

contains one-tenth more oxygen than yellow

2. When 100 grains of this oxide, prepared from red lead by nitric acid, are exposed to a red heat, they lose nine grains

of

of their weight, and are converted into yellow oxide: These oxide; easily
 nine grains are oxygen gas. Hence brown oxide is composed ^{disengaged.}
 of 91 yellow oxide and 9 oxygen. But 91 of yellow oxide
 contain 9.4 of oxygen. Therefore 100 parts of brown oxide
 are composed of

81.6 lead,
 18.4 oxygen.

100.

3. Mr. Proust, from his experiments, states the proportion
 of oxygen in this oxide at 21 *per cent.* If we take the mean
 of the two results, we obtain 19.7. We may, therefore, lay
 down 20 *per cent.* as the proportion of oxygen in brown oxide
 of lead: This cannot deviate far from the truth.

IV. Red Oxide.

Red lead being one of the most common of pigment, is un-Red lead;
 known, I presume, to no person. The method of manufac-
 turing it has been described by Dr. Watson in his *Chemical*
Essays, by Jars in the *Memoires* of the French Academy for
 1770, and by Ferber in his *Mineralogy* of Derbyshire.

1. It is a tasteless powder, very heavy, and of an intense its characters;
 red colour, often inclining to orange. I have never met with
 any specimens of it absolutely pure, but not unfrequently the
 foreign bodies do not exceed one or two *per cent.* They
 consist of seven grains of sand and oxide of antimony. Dr.
 Watson found traces of silver in it. It loses no sensible weight
 in a heat of 400°.

2. When 50 grains of red lead are digested in diluted nitric contains 88 lead
 acid, they leave 12 grains of brown oxide. The solution eva- + 12 oxygen;
 porated to dryness, yields 56 grains of nitrate of lead. Now,
 56 grains of nitrate contain 38.36 grains of yellow oxide.
 Red lead, therefore, is composed of 38.36 yellow oxide and
 12 brown oxide, or, *per cent.* of

76.72 yellow oxide,
 24.00 brown oxide.

100 72

The excess must be ascribed to the imperfection of our me-
 thods. I shall omit it in the calculation; Not that red lead

not easily de-
composed by
mere heat.

3. It is well known that red lead gives
when heated, and that it approaches to the
oxide: But, upon trial, I could obtain
this way. In one experiment 100 grains
per cent. in another seven per cent.
made in small covered earthen-ware
was melted into a dark-brown transpa-
rent glass of antimony, but much harder.
I found in it globules of lead reduced.
This accounts in part for the loss of weight.
contrary to the opinion of chemists, that
it is reducible, at least in part, by mere heat.
nothing prevents the complete reduction
which it unites with the vessel in which it
is melted. I twice varied the experiment, by enclosing
crucibles containing the red lead in a
but in neither case did I obtain any visible
yet the loss of weight was the same. It
that a portion of lead had been reduced
through the oxide.

Not soluble as
red lead in acids,
nor in alkalis.

4. The red oxide of lead does not
combine with acids. Many acids indeed
always begin by reducing it to the state
fixed alkalis do not alter its colour, but

Litharge consists of scales, partly of a golden yellow colour Litharge; and partly red: They possess a certain degree of elasticity. The method of making *litharge* has been described by Dr. Watson, by Gmelin, and by other chemical writers.

1. When 100 grains of litharge are exposed to a red heat, contains carbon-
they melt into a yellow glaze, and lose, at an average, four ^{nate,} grains of their weight. When 50 grains of pounded litharge are thrown into nitric acid, they dissolve with effervescence, and lose two grains of their weight. The effervescence and loss of weight are owing to the escape of carbonic acid gas. From this we may conclude, that litharge contains four *per cent.* of carbonic acid.

2. When 50 grains of litharge were dissolved in nitric acid, and antimony; and the solution evaporated to dryness and re-dissolved in water, $1\frac{1}{2}$ grains of a grey powder remained behind in my trials, which proved to be oxide of antimony. Therefore, litharge contains three *per cent.* of oxide of antimony.

3. The solution evaporated to dryness, gave 68.5 grains of Comp. parts in-
nitrate of lead; but this nitrate contains 46.72 grains of yellow ^{vestigated:} oxide of lead. Of course, we have litharge composed of

93.44 yellow oxide of lead,
3.00 oxide of antimony,
4.00 carbonic acid.

100.44

The small excess must be ascribed to unavoidable errors in the analysis.

4. Fifty grains of litharge dissolved in nitric acid, deprived of its oxide of antimony, and then thrown down by carbonate of potash, gave $52\frac{1}{2}$ grains of carbonate of lead. Hence 97 grains of litharge (supposing the antimony a foreign body) would have given 105 grains of carbonate. But 97 grains of litharge contain nearly four of carbonic acid. Hence we have the carbonate formed of 93 oxide and 12 acid.

In this experiment the carbonate produced was too small by about a grain. This was partly owing to the loss of a small quantity of the powder while separating it from the filtre. As I could not estimate the loss, I left it out in the calculation, and stated the amount precisely as I found it.

From the preceding experiments it follows, that litharge is a ~~it is a subcarbonate~~ sub-carbonate of lead, since it consists essentially of about 96 ^{nate:}

96 yellow oxide,
4 carbonic acid.

100.

Probably it varies somewhat in the proportion of its constituents, according to circumstances: But all my trials were made on one parcel of litharge. I have observed traces of carbonic acid also occasionally in red lead, but too little to affect its weight.

VI. Conclusions.

Summary recapitulation.

From the preceding experiments and observations we are entitled, I think, to draw the following conclusions:

1. Three oxides of lead only are at present known. The constituents of these oxides may be seen in the following table:

Oxides.	Colour.	Constituents.	
		Lead.	Oxygen.
Protoxide.	Yellow.	90.5	9.5
Deutoxide.	Red.	88	12
Peroxide.	Brown.	80	20

Lead. Oxygen.

$100 + 10.6 = 110.6$ protoxide.

$100 + 13.6 = 113.6$ deutoxide.

$100 + 25. = 125.$ peroxide.

2. The ashes of lead are a mixture of protoxide and lead in powder.

3. White lead and litharge are combinations of protoxide with carbonic acid: the first is a carbonate, the second a sub-carbonate of lead.

* As colour is a very ambiguous criterion for distinguishing metallic oxides, I have been accustomed for some time to denote the oxide with a minimum of oxygen by prefixing the Greek ordinal number to the term oxide: Thus, *protoxide of lead* is lead united to a minimum of oxygen. The oxide with a maximum of oxygen I call *peroxide*: Thus, *brown oxide of lead* is the *peroxide* of lead. I denominate the intermediate degrees of oxidizement by prefixing the Greek ordinals 2d, 3d, 4th, &c. Thus, *deutoxide* is the second oxide of lead, *tritoxide* of cobalt, the third oxide of cobalt, and so on.

4. The yellow nitrate of Proust contains the same oxide as common nitrate. But in it there is no excess of acid; whereas common nitrate is in fact a *supernitrate* of lead. In a strong heat it is partly converted into a *subnitrate*.

5. Protoxide of lead unites with all acids, deutoxide with none, and peroxide only with hyperoxymuriatic acid.

6. The protoxide of lead may be formed by combustion; but the other two cannot, and indeed lose oxygen in a strong heat. The deutoxide is formed by keeping protoxide in contact of air at a given temperature: the peroxide by the action of nitric or oxymuriatic acid on the deutoxide.

XI.

On certain Chemical Effects of Light. In a Letter from Wm. HYDE WOLLASTON, M.D. F. R. S.

To Mr. NICHOLSON.

SIR,

HAD I foreseen the publication of Mr. Ritter's '*Experiments on Light*' in the last number of your Journal,* I would have requested you to accompany them with a few observations of mine on the same subject; not with a view of claiming any priority in the observation of those invisible rays, that have chemical effects, which I believe occurred to Mr. Ritter and myself very nearly at the same time; but for the purpose of inserting a caution against the theory implied by the term "*dis-oxidating*" as applied to those rays.

Introduction on the invisible rays that have chemical effects.

In my note upon a communication to the Royal Society,† which you did me the honour to reprint in the 4th Vol. of your Journal, ‡ I was careful to express the power exerted by the most refrangible rays on muriate of silver, in general terms as *chemical*, not merely from a doubt whether they would in other cases produce a corresponding effect, but because I had at that time made the following experiments, which proved that the same rays, which cause the emission of oxygen by muriate of silver, occasion its absorption by the resin usually called gum guaiacum.

The power of these rays was termed chemical by Dr. W. and not dis-oxidating, because they do not constantly disoxidate.

* Page 214. † Phil. Trans. 1802, p. 379. ‡ 8vo. series. p. 99.

My

My reasons for withholding these experiments at that time were, that they appeared somewhat irrelevant to the primary subject under discussion, and that I was also in hopes of increasing their value by additional trials on other substances.

Whether the solar rays tend to extinguish fire; —not proved.

Upon considering the power which these rays possess of expelling oxygen from the muriate of silver, I thought it not impossible, that there might be more truth than I had been accustomed to suppose, in the popular observation that the sun has a tendency to extinguish fire, as the same rays might retard combustion by opposing the absorption of oxygen. Accordingly I made various experiments on different substances in a state of slow combustion, but without any apparent confirmation of that hypothesis.

Light or solar rays did not affect vegetable blues;—

I also tried the action of light on several vegetable blue colours, which are known to be affected by union with oxygen, and upon the same colours previously reddened; but on these also I did not succeed in producing any effect at either boundary of the prismatic spectrum.

Trial of guaiacum.

After failure of these endeavours, I had recourse to guaiacum, which I had long known to acquire a green colour by exposure to light; but that the presence of air is also requisite for this purpose I had ascertained in the following manner.

No change by solar rays without air.

Two plates of glass were heated with a small piece of guaiacum interposed, and thereby cemented together in their centers for a circular space about $1\frac{1}{2}$ inch diameter. In this state they were exposed for several weeks during summer to the sun, without the smallest apparent alteration in the colour of the guaiacum.

—but when the air had access;

The plates were then forcibly separated; and as they were both similarly coated with a portion of the resin adhering to their surfaces, one of them was preserved for comparison in a dark place, where it had free access of air alone, while the other was again exposed uncovered to the mid-day sun.

—it was affected in five minutes by the noon-day sun.

The latter was in five minutes perceptibly rendered green, and in a few hours had acquired the full colour, which it seemed capable of receiving; but the former, in the course of many months that it was kept confined from the light, seemed not to have been discernibly altered.

The prismatic spectrum was too weak to show whether all the rays were effective or the contrary.

Since by later experiments it appeared probable, that the whole of the sun's rays were not active in this process, with a view to determine on what part of them the effect might depend,

pend, I dissolved some guaiacum in alcohol, and after having washed a card with the tincture, I exposed it for some time to different parts of the common prismatic spectrum, but without producing any apparent change. It therefore became necessary to have recourse to other expedients for increasing the power of the spectrum.

Over the surface of a lens 7 inches in diameter, was pasted a circular piece of paper having its radius $\frac{1}{6}$ of an inch less than that of the lens. I had consequently remaining uncovered a prismatic annulus, corresponding in the length of its circumference to a prism 22 inches long, so arranged by its circular form that any one of the colours might at pleasure be brought to a focus, or the spectrum might be received as a ring of any diameter required, by mere variation of the distance of the lens. At short distances the exterior margin of the spectrum of course was red, and the violet within. The focus of brightest illumination was at $24\frac{1}{4}$; at greater distances the spectrum again became an annulus with its colours in an opposite order to the preceding, having the violet on the exterior margin.

With this apparatus the effect produced on muriate of silver is much accelerated. At distances short of $22\frac{1}{2}$ inches a ring is produced; at $22\frac{1}{2}$ a circular dark coloured spot; and at about 23 inches appears to be the focus of these rays, as the spot is then smallest; at $23\frac{1}{2}$ it is larger, at $24\frac{1}{4}$ it again becomes a ring shaded to the center; and at $24\frac{1}{2}$, (unless the paper has been wetted,) the center remains completely white though strongly illuminated. I have not however been able in any situation to restore the white colour to muriate of silver, after it has once been tinged, however slightly, by exposure to the most refrangible rays.

The experiments on guaiacum nevertheless will prove distinctly, that the powers of the two extremities of the spectrum are not only different, but opposite in their chemical effects.

A sufficient quantity of paper having been tinged with the solution of guaiacum, was cut into small pieces, some of which were exposed to the sun-shine till rendered completely green; the rest were kept confined from the light till taken out for each experiment.

The first endeavour was to ascertain the focal distance of those rays which gave the deepest colour in a given time; and it was found to be about 23 inches distant from the lens. At a short focal distance (namely of rays too at refrangible) the deepest green was produced;—

A broad convex lens being covered all but a narrow exterior ring, gave, at pleasure, according to the distance, a focal spot of any one colour, or on an annular spectrum, &c.

Muriate of silver very speedily affected by these concentrated rays;—

but not restored to whiteness.

Guaiacum was affected both ways.

Shorter rays produced the deepest green;—

shorter distances the surface coloured was larger, but in the same time paler. At $22\frac{1}{2}$ a green ring was formed, having its center without colour.

At a medium focus no effect was produced;

When the guaiacum was exposed at distances greater than 23 or $23\frac{1}{4}$, the surface coloured was also larger, but much paler than at equal distances short of the focus; inasmuch that at $24\frac{1}{2}$, which corresponds with the principal focus of illumination, little or no effect was produced in the space of one minute, which was the time employed in other experiments. It was manifest therefore, that the chemical effect of the most refrangible rays (which were now diverging beyond their focus;) and as it appeared probable that the power, which could in one instance prevent discoloration, might also, when duly applied, remove the same colour after it had been produced.

At a longer focal distance (namely of rays least refrangible) the original yellow was restored.

I next exposed to the condensed spectrum, at various distances from the lens, portions of the paper that had been previously rendered uniformly green. A second focus was now found at the distance nearly of $25\frac{1}{2}$, in which the green colour was completely removed, and the guaiacum restored to its pale yellow colour.

It is unnecessary to describe minutely the consequences resulting from variations of the distance, as the effects in this instance were necessarily the counterpart to the preceding; the circle of yellow was larger, when the paper was placed at a greater distance from the lens, and at the distance of $25\frac{1}{2}$ the center remained green, surrounded by a yellow ring, corresponding nearly to the red and orange interior margin of the annular prismatic spectrum.

In carbonic acid gas the disoxidation, but not the oxidation was practicable.

The same experiments being afterwards repeated in carbonic acid gas, only confirmed the opinion before entertained, of the causes to which the changes of colour were owing, but afforded little additional information. In this gas the guaiacum could not be rendered green at any distance from the lens, but was speedily restored from green to yellow by exposure to the focus of red rays.

The removal of colour (or disoxidation) was also effected by heat from a piece of metal.

Since the removal of colour was observed to take place in the situation of the principal focus of heat, it seemed desirable to ascertain whether the presence of light, or the circumstance of radiation had any influence in promoting this effect. A piece

piece of paper was therefore stained with guaiacum and after being rendered green by exposure for a sufficient length of time to light, was pressed on its posterior surface with a silver spoon previously heated over the flame of a candle; and the green colour was thereby as effectually removed, as in the focus of solar heat.

The last experiment may possibly appear to have been unnecessary; but until it is explained why the heat, that accompanies the sun's rays, penetrates the substance of transparent or semi-transparent bodies, while the radiant heat from a fire has scarcely power to enter even the most transparent, but principally scorches the surface, and is thence slowly conducted into the interior parts: no degree of caution upon a subject so imperfectly understood, should be deemed superfluous.

Radiant solar heat penetrates transparent bodies; culinary radiation scarcely at all.

I remain, Sir, &c.

W. WOLLASTON.

XII.

*Illustrations of Mr. Dalton's Theory of the Constitution of Mixed Gases. In a Letter from Mr. WM. HENRY, of Manchester, to Mr. Dalton. Communicated by the Writer.**

To Mr. DALTON.

DEAR SIR,

IN the first enunciation of a new theory, it is not unusual that some links are omitted in the chain of reasoning, which led to its formation; and thus the doctrine fails of that ready and general acceptance, which immediately follows its more distinct development. Such an omission appears to me to have taken place in your Theory of the Constitution of Mixed Gases; for, according to your own candid confession, several persons, versed both in chemical and mechanical science, have declared their inability fully to understand the scope of the hypothesis, and consequently to judge of its merits or defects. In the discussions also, which took place in this Society, on your several papers, the doctrine was opposed by almost every member interested on such subjects, and by no one more strenuously

Causes why a new theory may not readily produce conviction and adoption.

* Read before the Manchester Society.

nuously

nously than myself. Subsequent attention, however, to the evidences of the theory, and still more the results of experiments which were made under impressions very unfavourable to the hypothesis, have satisfied me that the opposition to it arose chiefly from an imperfect comprehension of the argument; and that your theory is far better adapted than any former one, for explaining the relation of mixed gases to each other, and especially the connection between gases and water.

Distinguishing
principle of
Dalton's theory.

The distinguishing principle of your doctrine I apprehend to be, that *mixed gases neither attract nor repel each other, and that every gas is as a vacuum to every other gas.* It is not my intention to recapitulate your proofs of this position, but merely to add to them the evidence of a few facts, which have occurred to me, and which strongly tend to establish the same conclusion.

Since gases are
held in water
(always) mechan-
ically in quan-
tities as the
pressure, differ-
ent gases do not
press each other
if they do not
prevent escape.

From a series of experiments, which I communicated to the Royal Society, and which appeared in their Transactions for 1803, it may, I think, be safely inferred, that the relation of gases to water is altogether a mechanical one; for the quantity absorbed follows exactly the ratio of the pressure. If then it can be shewn that a gas, absorbed by water, is not retained in its place by an atmosphere of any other gas, we shall be furnished with a strong presumption that different gases do not gravitate on each other.

Carbonic acid
quits water ex-
posed to the at-
mosphere,

It is well known that water may be charged with its own bulk, or rather more, of carbonic acid gas, under a pressure of 30 inches of mercury. The gas, thus absorbed, is retained so long as the water is preserved from contact with any other gas; but, when exposed to the atmosphere, the carbonic acid gas rapidly escapes. Now this effect can be only ascribed to one of two causes, 1st, the affinity of carbonic acid for atmospheric air may surpass that of its affinity for water; or, 2dly, the air of the atmosphere does not press on the gas in the water, which is therefore placed under similar circumstances, as if exposed under the exhausted receiver of an air-pump.

—because at-
tracted, or else
because not
pressed by the
atmosphere.

Not from at-
traction; for
atmospheres of
different gas
cause no differ-
ence.

Were the first explanation the true one, it might be expected that equal quantities of various gases would detach different quantities of carbonic acid from like volumes of impregnated water; because the affinities of these gases, as in all other cases of chemical affinity, differing in force, would occasion their combining with different quantities of carbonic acid, and in a certain

certain order. But on making the experiment, with all the attention I could bestow, this did not prove to be the fact: for similar measures of impregnated water gave up equal bulks of carbonic acid, to like quantities of all the different gases.

The reverse of this fact also occurred to me in the course of a series of experiments, to which I have already referred; viz. that the admixture of common air with carbonic acid gas diminishes considerably the proportion of the latter gas taken up by water. Thus, when 20 measures of pure carbonic acid gas are agitated with 10 of water, at least 10 measures of gas are absorbed. But from a mixture of 20 measures of carbonic acid with 10 of common air, 10 parts of water take only 6 of carbonic acid. That chemical affinity between the mixed gases is not the cause of the diminished amount of absorption, is perfectly clear; since it is indifferent, as to the effect, what gas is added, and the proportion alone influences the result. The effect is therefore to be ascribed to the diminished density of the superincumbent carbonic acid by mixture with another gas; and the pressure of gases being directly as their density, and the quantity absorbed by water being as the pressure, the absorbed carbonic acid must necessarily quit the water. This escape continues till the carbonic acid *above* the water has a density equal to that *in* the water, and no longer.

Previously to my acquiescence in your theory of mixed gases, I undertook an extensive series of experiments, with a view to ascertain the order of affinities of gases for water. But, after a great variety of trials, made with all the accuracy in my power, I could discover nothing like a series of elective attractions. Each gas, it was found, displaced every other, and reciprocally was dislodged by them.

It may be urged against the doctrine of the non-gravitation of gases on each other, that from water impregnated with carbonic acid gas, and exposed to the atmosphere, the gas ought, on this principle, to escape as rapidly as under an exhausted receiver. It must be remembered, however, that the escaping gas constitutes, by admixture with the air of the atmosphere, a gas of diminished density, but still of such density as to retard the escape of farther portions. All that the air-pump effects is to remove these as fast as they are liberated.

There are various facts, satisfactorily explained on this doctrine, which are irreconcilable to any former hypothesis. Of these

If carbonic acid be mixed with another gas, the absorption by water is governed by the density of the carbonic acid and not by the mixture of the gas

Water has no preference of attraction as to the gases.

The slower escape of a gas from water exposed to the atmosphere compared with a vacuum arises from the less rapid exhaustion of the superincumbent gas.

Facts explained by Dalton's theory.

these I shall mention only a few; since the theory will receive from yourself all the elucidation that its establishment can require.

Light and heavy
gases mix spon-
taneously.

1. If each gas be a vacuum to every other, a heavier gas should ascend into a lighter one, without the aid of agitation; and on the contrary a lighter one should descend into a heavier one. That this is actually the fact, and under circumstances very unfavourable to their mixture, your own experiments have fully proved.

Sulphuret takes
oxygen from air
not agitated.

2. The hypothesis explains why sulphuret of potash withdraws oxygen from the air without agitation, and whether placed at the top or at the bottom of a jar; for it acts as if the absorbed gas were the only one present in the vessel.

Aborbable gases
expel the last
portions of com-
mon air from
water.

3. It explains why the last portions of common air are expelled from water by carbonic acid, and other aborbable gases. For these gases act as a vacuum to the air contained in the water, which must therefore necessarily quit its place. It solves also the problem how to expel completely any gas from water; for to effect this, the water must successively be agitated with portions of some other gas of the greatest attainable purity. Thus to expel atmospherical air entirely from water, it may be agitated with pure carbonic acid gas; but as the liberated common air presses on that remaining in the water, according to the proportion it bears to the superincumbent carbonic acid, the gas thus employed must be removed, and fresh and pure portions used in succession.

Best method of
impregnating
water with a gas.

4. By applying the same general law, we are taught how to effect the highest attainable impregnation of water with any gas. There could be no difficulty in accomplishing this object, if the gas and water were both absolutely uncontaminated by admixture with other gases; but when pure carbonic acid is agitated with water, atmospherical air is extricated, which, mingling with the carbonic acid, lessens its density. To obviate this difficulty as much as possible, a quantity of water, to be impregnated fully with carbonic acid, should be agitated with several successive portions of the purest possible gas. The unabridged residuum should also be very large, in order that the carbonic acid may bear a large proportion to other aeriform substances accidentally mixed with it.

These are, doubtless, only a few of the phenomena, to the explanation of which your theory may be successfully applied; and

and I confidently expect that many facts, hitherto referred to chemical principles, will be brought, in consequence of your discoveries, within the pale of mechanical philosophy.*

I am, Dear Sir,

Your's very truly,

WILLIAM HENRY.

Manchester, June 20, 1804.

XIII.

On the Disappearance of Oxygen and Hydrogen over Water, at the Heat of the Atmosphere. By T. S. T.

To Mr. NICHOLSON.

SIR,

SOME months ago, I read in your excellent Journal an account of an experiment, which tended to shew that oxygenous and hydrogenous gases, when mixed together, and allowed to remain over the surface of water for a long time, spontaneously united and formed water.

Slow absorption
of oxygen and
hydrogen over
water.

Having long been accustomed to consider a temperature considerably higher than that our atmosphere ever attains, necessary to this union, I was naturally led to investigate this phenomenon; and for that purpose undertook the following experiments:

1. I prepared oxygen gas from black oxide of manganese, by means of concentrated sulphuric acid, aided by heat, and in order to render it more pure, I washed it well with milk of lime. I prepared likewise a quantity of hydrogen gas, by passing a few drops of water through a gun-barrel, filled with iron filings, and passed through the body of a small furnace, I introduced nearly equal quantities for both gases into a bell-glass jar, placed on the shelf of a common pneumatic trough, which stood in a room without fire, and almost without light. The mixture was suffered to remain in that situation for about five months; at the end of which time, the volume of the gases had diminished $\frac{1}{3}$.

* As the author had not seen Mr. Dalton's letter, published in our last number, at the time when these illustrations were written, he has mentioned a few circumstances contained in the letter.

Slow absorption
of oxygen and
hydrogen over
water.

2. On reading the account above alluded to, it occurred to me that it was possible, that the diminution in bulk, might have arisen from a partial absorption of one or of both gases, by the water of the trough. In order to ascertain this, I introduced like proportions of both gases, into a jar, placed in a mercurial trough, which was in the same room with the other; and after suffering this experiment to continue as long as the other, I found that of 12 cubic inches of both gases introduced into the jar, $3\frac{1}{2}$ had disappeared; but I could scarcely perceive any moisture on the sides of the jar, owing to the small quantity of water which had been formed.

The decrease in volume, in these experiments, could not be owing to any condensation of the gases, by the coolness of the surrounding air; for I found that it took place gradually; and the mixtures were made in the beginning of January, and stood till the end of May; consequently there should have been rather an increase than a decrease in bulk, if the temperature of the air was the cause.

The residuary air contained in the jars, still consisted of oxygen and hydrogen gases; for when received into a phial, on the application of a lighted taper, a smart explosion took place, and the sides of the phial grew dim. On adding sulphuret of lime to another portion of the residue, a rapid absorption of the oxygen gas took place, and hydrogenous gas was left behind.

From these facts we may fairly conclude, that the decrease in volume was owing to the spontaneous combination of the two gases to form water.*

T. S. T.

Orkney, June 20th, 1804.

* It deserves to be considered whether the absorption of the purer gases within, and the escape at the surface of the water exposed to the atmosphere, according to the doctrine explained in Mr. Henry's paper (page 297) may not have occasioned the deficiency.

W. N.

XIV.

Letter from Dr. P. A. NEMNICH, expressing Doubts with regard to the Death of the celebrated Humboldt.

To Mr. NICHOLSON.

SIR,

Hamburg, June 29, 1804.

YOUR Journal of Natural Philosophy, &c. June 1, 1804, page 72, mentions positively the death of Mr. Humboldt, which, as we had here in Germany no other advices, I immediately communicated to my countrymen through the channel of our newspapers. There are however in Germany, as well as in France, many doubts about the validity of the said notice, and many objections made. Having quoted your Journal, as above-mentioned, I should be very much obliged to you for a more circumstantial and positive account of this report of Mr. Humboldt's death, with the day of his decease, and the way by which this notice reached England, &c. which as soon as I have received, I will instantly make public, in order to maintain that credibility your valuable Journal deserves.

I am, Sir,

Your's most respectfully,

P. A. NEMNICH,
Licentiate.

Extrait du Publiciste.

Paris, 20 Juin, 1804.

VOTRE feuille de ce jour contient à l'article de Hambourg la nouvelle de la mort de Mr. de Humboldt. Il m'est permis d'en révoquer en doute l'authenticité, et de rassurer les amis des sciences et de l'humanité. Je sais positivement que Mr. Guil. de Humboldt à Rome, a reçu de son frere des lettres datées de la Havane du 28 Mars, dans lesquelles il lui marquoit que sous 12 jours il seroit rendu à Charlestown d'où il s'embarqueroit de suite pour le Havre, et qu'il comptait être à Paris avant la fin de juin.—La nouvelle est donc plus que douteuse et nous pouvons esperer, que le sort ne se seroit pas fait un jeu cruel de rendre vain le dévouement sans bornes et les nobles efforts de l'illustre voyageur. (Signé) Mendelssohn.

Extra from the Publiciste.

Paris, 20th June, 1804.

Translation.

YOUR number of this day contains under the article *Hamburgh*, an account of the death of Mr. de Humboldt. I have reasons to doubt the authenticity of this article, and to encourage the friends of science and of humanity. I know positively that Mr. William de Humboldt received from his brother letters dated from the Havannah, of the 28th March, in which he informs him that in twelve days he should go to Charlestown and embark for Havre, with the expectation of arriving in Paris before the end of June. Your article of news is therefore more than doubtful, and we may hope that the course of events have not been so unfavourable as to render the unlimited sacrifices and efforts of this illustrious traveller of no use to society.

* * Dr. Gibbes, the author of the note in question, will, no doubt, have the goodness to mention his authority, when he sees this.

W. N.

✂ From the extraordinary Number of valuable Communications this Month, (every one of the Articles, but those of GAY LUSSAC and BARTHOLDI, being original) it has been necessary to postpone an excellent Memoir on Haüy's System, by the Abbé Briel, and a Paper by E. O. on the Computation of Tables of Squares and Cubes, both which, and some Abridgements and Collections of interesting Matter from the *Phil. Transactions*, together with the *Scientific News*, will appear in our next.

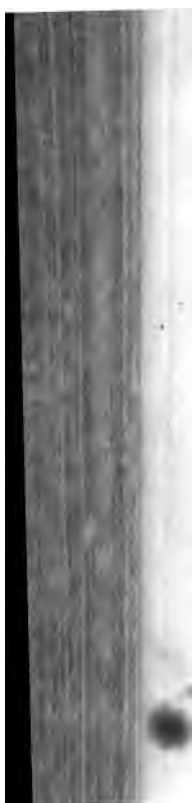
Antique painting composed in glass.

Fig. 2.



Fig. 1.





Improved Chemical Lamp.

Fig. 1.



Fig. 2.

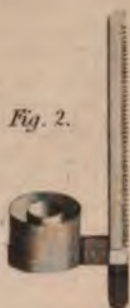


Fig. 3.



1 Foot.



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THE END OF THE EIGHTH VOLUME.







